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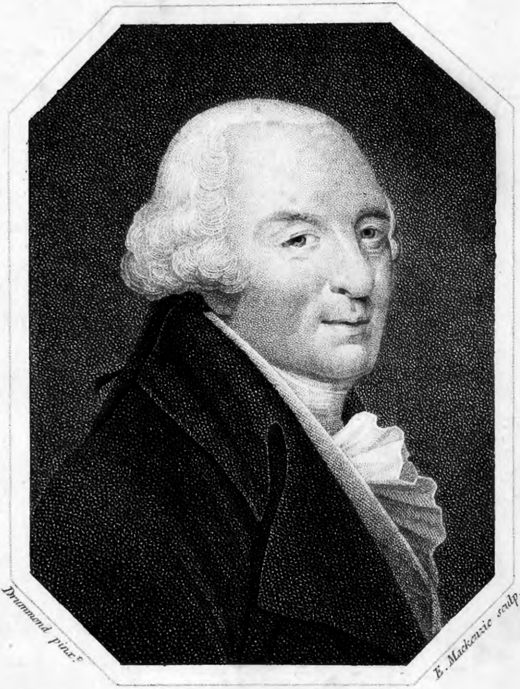

ALEX.R AUBERT ESQ.R F.R.S.

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THE
PHILOSOPHICAL MAGAZINE:

COMPREHENDING

THE VARIOUS BRANCHES OF SCIENCE,

THE LIBERAL AND FINE ARTS,

AGRICULTURE, MANUFACTURES,

AND

COMMERCE.

BY ALEXANDER TILLOCH,

HONORARY MEMBER OF THE ROYAL IRISH ACADEMY, &c. &c. &c.

" Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut apes." JUST. LIPS. *Monit. Polit.* lib. i. cap. 1.

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1806.

CONTENTS

OF THE

TWENTY-FIFTH VOLUME.

CONTENTS.

XXX. *Pro-*

CONTENTS.

XLVII. *Prin-*

CONTENTS.

THE

THE

PHILOSOPHICAL MAGAZINE.

I. *On Waters distilled from inodorous Plants* *.

Every plant whatever has a decided smell peculiar to itself; in some, however, it is so feeble that it is scarcely perceptible. This is certainly the reason why at one time plants were divided into odorous and inodorous: the former were regarded as enjoying many more properties than the latter; and for this reason, much more confidence was placed in those medicines prepared from odorous than from inodorous vegetables.

It must be admitted that the opinion formed on this subject is not absurd; in fact, it is impossible to lay aside the idea, that a substance which affects the organ of smelling in a remarkable manner, should also produce upon the animal œconomy a more remarkable influence than a substance which emits little or no smell: as a necessary consequence of this reasoning, we ought to conclude that waters distilled from inodorous plants have no virtue, and do not differ from common distilled water. Many medical men are even so convinced that the above ideas are correct, that they never prescribe such waters as may be distilled from inodorous plants, but always substitute infusions or decoctions of them.

Some particular observations, however, seem to ascertain that the kind of proscription which has been denounced against waters distilled from inodorous plants is ill founded; and, if they have been often remarked to have no effect, it has been because the requisite precautions were not used in their preparation.

It remains, then, to ascertain the mode of preparation

* From *Journal de Medecine.*

necessary to be adopted, and in this the chief object to be attended to is to collect as much as possible of the *aroma* of the plants which are submitted to distillation. Several experiments were made with this view, and they were attended with the most happy results.

Among all the processes employed, that of cohobating and recohobating the first distilled water of the plant upon fresh quantities of the same plant seemed to be the most successful. In fact, it was thought that, an inodorous plant only containing a small quantity of aroma, the first water distilled could never be very rich in the odorous principle, since the plant could only furnish the small quantity belonging to it; but if the distillation is repeated with the first distilled water and a new quantity of plants, the produce of this second distillation ought necessarily to have more odour than the first; and for the same reason, by means of a third distillation, and even a fourth, we may be able to saturate the water with aroma, which then not only acquires a sensible taste and smell, but also acts differently when used for medicinal purposes from the water obtained by one distillation only.

For several years past I have prepared distilled waters by the above method, and I always obtained results conformable to the above theory. Latterly I have added new proofs to those I had already acquired, and the establishment of the imperial laboratory, in particular, furnished me with these opportunities. Being under the necessity of providing all the medicines for that institution, and also of attending to their preparation with the utmost exactitude, I invited M. Clairon, my assistant in the laboratory, to lend me his aid in the project I had conceived of distilling, in the most careful manner, such plants as are thought inodorous. We stopped the operation from time to time, in order to introduce such modifications and improvements as occurred to us from reflections during the operation. Consequently, twenty-five inodorous plants were first distilled in the usual way; the produce of each distillation was afterwards cohobated, and recohobated upon fresh portions of plants; and we always stopped the operation when we discovered

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covered that the produce which came from the beak of the alembic began to differ from that first distilled. Frequently we even submitted the water we had thus repeatedly distilled, to a distillation in B. M., and drew off only a small portion of it for the purpose of ascertaining if by this means we could obtain an article richer in the odorous principle; and, finally, we had sometimes recourse to four successive cohobations with the same view. The following is the conclusion we formed:

Three cohobations were sufficient to saturate the water with all the aroma of the plants distilled; after which, further distillations were of no service, as the produce was not increased in the odorous quality.

All our distilled waters had a peculiar taste, and the smell of the plants made use of: the latter was very strong. Some few of them were muddy, but the greatest part of them were perfectly transparent. One thing we remarked which appeared very extraordinary to us; namely, that the smell of several of these waters was so very pungent, that one would have thought some of them had been prepared from cochlearia or horse-radish root. I shall particularly notice, by way of example, the water distilled from the flower of little centaury. We know that this flower, even when fresh gathered, has scarcely any perceptible odour: upon being distilled, however, it furnished us with a smell so pungent that we could hardly support it. This water, upon the third distillation, came off thick, and we perceived upon its surface some molecules of a thickish oil of a white colour, having a very bitter pungent taste.

Waters prepared in the manner I have stated, are not all equally preserved in good condition: some of them soon undergo an alteration: this is the case, in particular, with water of borage. M. Clairon had occasion to remark, that fifteen days after its preparation it began to lose its transparency, and a heap of mucous threads was formed in the middle of it, which very soon fell to the bottom and appeared like a very slender magma, which was set in motion by the smallest shaking of the vessel: the water then ac-

quired a very disagreeable smell, a little similar to that of an animal substance which had begun to putrefy. I also particularly observed that this effect was more evident when the water was kept in transparent bottles exposed to the light, than when kept in such as were opake and placed in the dark; and lastly, that by separating, by the filter and syphon, the magma which was formed, and afterwards exposing the water for some hours to the air, the putrid smell was entirely got rid of, and that of the borage was restored. I shall not now speak of the cause which determines the putrefaction of some distilled waters, but there is one circumstance which I ought not to omit; namely, that all waters which are subject to alteration should not only never be kept in transparent vessels exposed to the light, but that such vessels should never be sealed hermetically.

The duration of waters distilled from inodorous plants is rarely more than a year. At this age the smell becomes considerably weaker, and soon afterwards it disappears almost entirely. A careful apothecary ought therefore to renew his stock of distilled waters every year: without this precaution their effects in medicine cannot be depended on.

But to return to the question now before us, whether waters distilled from inodorous plants have such decided properties as to entitle them to be introduced into practice. Upon this subject I think there can be no possibility of doubt, if we admit that aroma in general, whatever is the nature of the substance which supplies it, acts in a certain manner upon the animal œconomy: thus, as it is proved that we can procure the aroma of any plant, however inodorous, in such a quantity as to make it perceptible to the smell, it must necessarily follow that the water thus impregnated will produce effects proportioned to the quantity and quality of the aroma it contains. In short, the above reasoning is completely proved by actual experiments.

Of several distilled waters, the effects of which I have ascertained, I shall content myself here with describing the properties of the water distilled from lettuce. How often have we heard that this water enjoyed no property whatever! It

It is nevertheless certain that its properties are very remarkable, particularly when it is prepared by successive cohobations. I know a lady who is extremely nervous, and who never fails, when she has occasion for a sedative, to take, at bed-time, two or three ounces of well distilled lettuce water: this has always the same effect as liquid laudanum, which she had formerly used in great quantities.

But if the water distilled from lettuce possesses some influence, why may not the same result from all the other inodorous plants? Thus, when I hear it maintained that the waters produced from centaury, argentine, pellitory, purslane, orpine, &c. ought to be dismissed from our dispensatories, I conclude that those who are of this opinion have never employed these waters when properly prepared, or have not accurately noticed their effects.

Waters distilled from inodorous plants are not only useful in medicine, but in the arts also they are often employed to advantage.

I shall take the present opportunity of quoting an observation communicated to me by M. Desprès, a distinguished chemist of Paris, and to whom we may pay so much the more credit, as that gentleman adds to an extensive knowledge, the most scrupulous probity and the most perfect correctness. I understand from him, that not only has he successfully employed as medicines the above kind of distilled waters, but that even the manufacturers of gauze, who are very numerous in Paris, could never give their gauzes that lustre, brilliance, and consistency, for which they are so eminent, but by macerating them in the distilled water of argentine (*pentaphylloides, argenteum alatum, seu potentilla*). M. Desprès added, that his servants, having substituted common distilled water in place of the above water, the manufacturers came to him to complain that they had been imposed upon. We must certainly admit that argentine is not a very odorous plant; nevertheless, if the water produced from it has a decided action upon the silk of which the gauze is made, we need not longer doubt that it acts also upon the animal œconomy, and that its effects ought to differ from those produced by common distilled water.

From all this it results:

1. That it is very ill judged to proscribe the medicinal use of waters distilled from plants called inodorous.

2. That these waters have decidedly constant properties.

3. That these properties are the more decided, the more they contain of the aroma of the plant.

4. That the proper method is to cohobate upon fresh plants thrice or even four times the produce first distilled.

5. That the waters thus procured ought always to be preserved in vessels not capable of being traversed by the rays of light.

6. That it is necessary to clear these waters from the flaky deposits which appear soon after distillation.

7. That, considering the short duration of these waters in a state of perfection, it is indispensably necessary for the apothecary to renew his stock of them once a year.

8. Lastly, that it is desirable that physicians, profiting by the facts which have been now laid before them, would turn their attention to the discovery of new ones. Their experiments on this subject will be extremely useful, because they will contribute to destroy the prejudice which maintained, that, waters distilled from inodorous plants having no action upon the animal œconomy, their use ought to be proscribed.

II. *Process for Dyeing the Adrianople or Turkey Red, as practised at Astracan. By Professor* PALLAS: *being a Supplement to his former Publications on that Art*.*

ONE of my friends, the proprietor of a dye-house at Astracan, communicated to me the most accurate details of the process for dyeing Turkey red. On Saturday, after the cotton yarn has undergone those preliminary preparations already mentioned*, it is immersed, for the first time, in fish grease or fish oil, which is made into a lather by a solution of soda: heaped up in this bath, where it heats sensibly, it

* See Philosophical Magazine, vol. i., and also vol. xviii., for the process used by Mr. Papillon, of Glasgow.

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is left till Monday, when it is washed, dried, re-immersed in this oily emulsion or saponaceous mass, and again suspended in the air. The same operation is repeated on Tuesday for the third time. The four following days it is washed four times in lye made of a solution of pure soda. After that it receives the first tint of olive green with the leaves of sumach (*rhus cotinus*): 123 lb. of these leaves (for which nut-galls may be substituted) to about 140 gallons of water, boiled in a cauldron, are sufficient for 330lbs. of cotton yarn. To this decoction, after being well boiled, strained, and the boiler cleaned of all extraneous matter, are added 33 lbs. of alum. The yarn is then divided into skains, and placed in little pots or saucers in this boiler and boiled; after which it is sufficiently prepared for the red dye. To prepare this bath, take 33 lb. of madder roots (*rubia tinctorum*), ground, to each 33 lbs. of yarn (less madder will do if it be of the best quality). The madder is then kneaded in about seven quarts of blood, with which it is well boiled in the cauldron: the yarn is afterwards immersed in this boiled colour, and the boiling continued until it is well penetrated by the colouring particles. The coloured stuff is then dried, and afterwards put into pots full of weak alkaline ley, in which it is slightly boiled. In this latter process the alkaline liquor is constantly kept of equal strength by the addition of fresh lye being regularly added. The cotton yarn, when cleaned and dried, is then found perfectly dyed. This series of operations generally continues 21 days. It is said that the Turks, to give it a brighter or deeper colour, finish by plunging it again in an oily emulsion, and that they leave it to dry under a press. They also use oil of olives instead of fish oil. In general, all unctuous or greasy fluids, that will lather perfectly with a solution of soda, are equally proper for this dye. The madder which has small roots is considered the best. Thirty-three pounds of kelp or crude soda (*kalakar*) are generally enough for a boiler containing 140 gallons of water. To dye 40 lbs. of cotton, take 15 lbs. of sumach, 4 lbs. of alum, 58 lbs. of fish oil, 40 lbs. of crude soda or kelp, 40 lbs. of madder, two cauldrons and four alkaline vats.

III. *Ac-*

III. *Account of a Series of Experiments, showing the Effects of Compression in modifying the Action of Heat.* By Sir James Hall, *Bart. F.R.S. Lond. and Edin.*

[Continued from our last volume, p. 307.]

V. *Experiments in which Water was employed to increase the Elasticity of the included Air.—Cases of complete Compression.—General Observations.—Some Experiments affording interesting Results; in particular, showing a mutual Action between Silex and the Carbonate of Lime.*

Finding that such benefit arose from the increase of elasticity given to the included air in the last-mentioned experiments by the diminution of its quantity, it now occurred to me, that a suggestion formerly made by Dr. Kennedy, of using water to assist the compressing force, might be followed with advantage; that, while sufficient room was allowed for the expansion of the liquid metal, a re-acting force, of any required amount, might thus be applied to the carbonate. In this view I adopted the following mode, which, though attended with considerable difficulty in execution, I have often practised with success. The weight of water required to be introduced into the barrel was added to a small piece of chalk or baked clay, previously weighed. The piece was then dropped into a tube of porcelain of about an inch in depth, and covered with pounded chalk, which was firmly rammed upon it. The tube was then placed in the cradle along with the subject of experiment, and the whole was plunged into the fusible metal, previously poured into the barrel, and heated so as merely to render it liquid. The metal being thus suddenly cooled, the tube was encased in a solid mass before the heat had reached the included moisture. The difficulty was to catch the fusible metal at the proper temperature; for when it was so hot as not to fix in a few seconds, by the contact of the cradle and its contents, the water was heard to bubble through the metal and escape. I overcame this difficulty, however, by first heating the breech of the barrel (containing a sufficient quantity of fusible metal) almost to redness, and then setting

ting it into a vessel full of water till the temperature had sunk to the proper pitch, which I knew to be the case when the hissing noise, produced in the water by the heated barrel, ceased; the cradle, during the last stage of this operation, being held close to the muzzle of the barrel, and ready to be thrust into it.

On the 2d of May I made my first experiment in this way, using the same air-tube as in the last experiment, which was equal in capacity to one-thirtieth of a cubic inch. Half a grain of water was introduced in the manner just described. The barrel, after an hour of red heat, was let down by a rope and pulley, which I took care to use in all experiments in which there was any appearance of danger. All was sound. The metals rushed out smartly, and a flash of flame accompanied the discharge. The upper pyrometer gave 24°, and the lower one 14°. The contents of the inner tube had lost less than 1 per cent., strictly 0·84. The carbonate was in a state of good limestone; but the heat had been too feeble: the lower part of the chalk in the little tube was not agglutinated: the chalk round the fragment of pipe-stalk (used to introduce the water), which had been more heated than the pyrometer, and the small rod, which had moulded itself in the boll of the stalk, were in a state of marble.

On the 4th of May I made an experiment like the last, but with the addition of 1·05 grains water. After application of heat, the fire was allowed to burn out till the barrel was black. The metal was discharged irregularly. Towards the end, the inflammable air produced, burnt at the muzzle, with a lambent flame, during some time, arising doubtless from hydrogen gas, more or less pure, produced by the decomposition of the water. The upper pyrometer indicated 36°, and the lower one 19°. The chalk which lay in the outer part of the large tube was in a state of marble. The inner tube was united to the outer one by a star of fused matter, black at the edges, and spreading all round, surrounding one of the fragments of porcelain which had fallen by accident in between the tubes. The inner tube, with the starry matter adhering to it, but without the coated fragment,

ment, seemed to have sustained a loss of 12 per cent. on the original carbonate introduced. But, the substance surrounding the fragment being inappreciable, it was impossible to learn what loss had been really sustained. Examining the little tube, I found its edges clean, no boiling over having taken place. The top of the small lump of chalk had sunk much. When the little tube was broken, its contents gave proof of fusion in some parts, and in others of the nearest approach to it. A strong action of ebullition had taken place all round, at the contact of the tube with the carbonate: in the heart the substance had a transparent granular texture, with little or no crystallization. The small piece of lump-chalk was united and blended with the rammed powder, so that they could scarcely be distinguished. In the lower part of the carbonate, where the heat must have been weaker, the rod had acted more feebly on the tube, and was detached from it: here the substance was firm, and was highly marked in the fracture with crystalline facettes. Wherever the carbonate touched the tube, the two substances exhibited, in their mixture, much greater proofs of fusion than could be found in the pure carbonate. At one place, a stream of this compound had penetrated a rent in the inner tube, which it had filled completely, constituting a real vein, like those of the mineral kingdom; which is still distinctly to be seen in the specimen. It had then spread itself upon the outside of the inner tube to the extent of half an inch in diameter, and had enveloped the fragment of porcelain already mentioned. When pieces of the compound were thrown into nitric acid, some effervesced, and some not.

I repeated this experiment on the same day with two grains of water. The furnace being previously hot, I continued the fire during one half-hour with the muffle open, and another with a cover upon it. I then let the barrel down by means of the pulley. The appearance of a large longitudinal rent made me at first conceive that the experiment was lost, and the barrel destroyed: the barrel was visibly swelled, and in swelling had burst the crust of smooth oxide with which it was surrounded: at the same time no exudation of metal had happened, and all was sound. The metals were thrown out

out with more suddenness and violence than in any former experiment; but the rod remained in its place, being secured by a cord. The upper pyrometer gave 27°, the lower 23°. The contents of the inner tube had lost 1·5 per cent. The upper end of the little lump of chalk was rounded and glazed by fusion; and the letter which I have been in the habit of cutting on these small pieces, in order to trace the degree of action upon them, was thus quite obliterated. On the lower end of the same lump, the letter is still visible. Both the lump and the rammed chalk were in a good semi-transparent state, shining a little in the fracture, but with no good facettes, and no where appearing to have acted on the tube. This last circumstance is of consequence, since it seems to show, that this very remarkable action of heat, under compression, was performed without the assistance of the substance of the tube, by which, in many other experiments, a considerable additional fusibility has been communicated to the carbonate.

These experiments, and many others made about the same time, with the same success, clearly prove the efficacy of water in assisting the compression; and results approaching to these in quality, obtained, in some cases, by means of a very small air-tube, show that the influence of water on this occasion has been merely mechanical.

During the following summer and autumn 1803, I was occupied with a different branch of this subject, which I shall soon have occasion to mention.

In the early part of last year (1804) I again resumed the sort of experiments lately described, having in view principally to accomplish absolute compression, in complete imitation of the natural process. In this pursuit I did not confine myself to water, but made use of various other volatile substances in order to assist compression; namely, carbonate of ammonia, nitrate of ammonia, gunpowder, and paper impregnated with nitre. With these I obtained some good results, but none such as to induce me to prefer any of these compressors to water. Indeed, I am convinced that water is superior to them all. I found, in several experiments, made with a simple air-tube, without any artificial compressor,

compressor, in which a very low red heat had been applied, that the carbonate lost one or one and a half per cent. Now, as this must have happened in a temperature scarcely capable of inflaming gunpowder, it is clear that such loss would not have been prevented by its presence; whereas water, beginning far below redness to assume a gaseous form, will effectually resist any calcination, in low as well as in high heats. And as the quantity of water can very easily be regulated by weight, its employment for this purpose seems liable to no objection.

On the 2d of January 1804, I made an experiment with marble and chalk, with the addition of 1·1 grain of water. I aimed at a low heat, and the pyrometer, though a little broken, seemed clearly to indicate 22°. Unluckily, the muzzle of the large tube, which was closed as usual with chalk, was placed uppermost, and exposed to the strongest heat. I found it rounded by fusion, and in a frothy state. The little tube came out very clean, and was so nearly of the same weight as when put in, that its contents had lost but 0·074 per cent. of the weight of the original carbonate. The marble was but feebly agglutinated, but the chalk was in a state of firm limestone, though it must have undergone a heat under 22°, or that of melting silver. This experiment is certainly a most remarkable one, since a heat has been applied in which the chalk has been changed to hard limestone with a loss less than the 1000dth part of its weight (exactly $\frac{1}{1351}$); while, under the same circumstances of pressure, though probably with more heat, some of the same substance had been brought to fusion. What loss of weight this fused part sustained, cannot be known.

On the 4th of January a similar experiment was made likewise with 1·1 grain of water. The discharge of the metal was accompanied with a flash of flame. The pyrometer indicated 26°. The little tube came out quite clean. Its contents had been reduced from 14·53 to 14·46; difference 0·07 grains; being 0·47 per cent. on the original carbonate, less than 1-200dth part of the original weight (exactly $\frac{1}{212}$). The chalk was in a state of firm saline marble, but with no unusual qualities.

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These two last experiments are rendered still more interesting by another set which I made soon after, which showed that one essential precaution in a point of such nicety had been neglected in not previously drying the carbonate. In several trials made in the latter end of the same month, I found, that chalk exposed to a heat above that of boiling water, but quite short of redness, lost 0·34 per cent., and in another similar trial 0·46 per cent. Now, this loss of weight equals within 0·01 per cent., the loss in the last-mentioned experiment, that being 0·47; and far surpasses that of the last but one, which was but 0·074. There is good reason, therefore, to believe, that had the carbonate, in these two last experiments, been previously dried, it would have been found during compression to have undergone no loss.

The result of many of the experiments lately mentioned seems fully to explain the perplexing discordance between my experiments with porcelain tubes and those made in barrels of iron. With the porcelain tubes, I never could succeed in a heat above 28°, or even quite up to it; yet the results were often excellent: whereas the iron barrels have currently stood firm in heats of 41° or 51°, and have reached even to 70° or 80° without injury. At the same time, the results, even in those high heats, were often inferior, in point of fusion, to those obtained by low heats in porcelain. The reason of this now plainly appears. In the iron barrels it has always been considered as necessary to use an air-tube, in consequence of which some of the carbonic acid has been separated from the earthy basis by internal calcination: what carbonic acid remained has been more forcibly attracted, according to M. Berthollet's principle, and, of course, more easily compressed, than when of quantity sufficient to saturate the lime: but, owing to the diminished quantity of the acid, the compound has become less fusible than in the natural state, and, of course, has undergone a higher heat with less effect. The introduction of water, by furnishing a reacting force, has produced a state of things similar to that in the porcelain tubes; the carbonate sustaining little or no loss

loss of weight, and the compound retaining its fusibility in low heats *.

In the early part of 1804, some experiments were made with barrels, which I wished to try, with a view to another series of experiments. The results were too interesting to be passed over; for, though the carbonic acid in them was far from being completely constrained, they afforded some of the finest examples I had obtained of the fusion of the carbonate and of its union with silex.

On the 13th of February an experiment was made with pounded oyster-shells, in a heat of 33°, without any water being introduced to assist compression. The loss was apparently of 12 per cent. The substance of the shell had evidently been in viscid fusion: it was porous, semi-transparent, shining in surface and fracture; in most parts with the gloss of fusion, in many others with facettes of crystallization. The little tube had been set with its muzzle upwards; over it, as usual, lay a fragment of porcelain, and on that a round mass of chalk. At the contact of the porcelain and the chalk they had run together, and the chalk had been evidently in a very soft state; for, resting with its weight on the porcelain, this last had been pressed into the substance of the chalk deeper than its own breadth, a rim of chalk being visible without the surface of the porcelain; just as when the round end of a knife is pressed upon a piece of soft butter. The carbonate had spread very much on the inside of the tube, and had risen round its lip, as some salts rise from their solution in water. In this manner, a small quantity of the carbonate had reached the outer tube, and had adhered to it. The black colour frequently mentioned as

* The retentive power here ascribed to the porcelain tubes, seems not to accord with what was formerly mentioned, of the carbonic acid having been driven through the substance of the tube. But the loss by this means has probably been so small, that the native properties of the carbonate have not been sensibly changed: or, perhaps, this penetrability may not be so universal as I have been induced to think, by having met with it in all the cases which I tried. In this doubt I strenuously recommend a further examination of this subject to gentlemen who have easy access to such porcelains as that of Dresden or of Seve.

accompanying

accompanying the union of the carbonates with the porcelain, is here very remarkable.

On the 26th of February I made an experiment, in which the carbonate was not weighed, and no foreign substance was introduced to assist the compression. The temperature was 46°. The pyrometer had been affected by the contact of a piece of chalk, with which it had united; and some of the carbonate must have penetrated the substance of the pyrometer, since this last had visibly yielded to pressure, as appeared by a swelling near the contact. I observed in these experiments that the carbonate had a powerful action on the tubes of Cornish clay, more than on the pounded silex. Perhaps it has a peculiar affinity for argil, and this may lead to important consequences. The chalk had visibly first shrunk upon itself, so as to be detached from the sides, and had then begun to run by successive portions, so as still to leave a pillar in the middle, very irregularly worn away; indicating a successive liquefaction, like that of ice, not the yielding of a mass softening all at once.

On the 28th of February I made an experiment with oyster-shell unweighed, finely ground, and passed through the closest sieves. The pyrometer gave 40°. The piece of chalk below it had been so soft, as to sink to the depth of half an inch into the mouth of the iron air-tube, taking its impression completely. A small part of this lump was contaminated with iron, but the rest was in a fine state. The tube had a rent in it, through which the carbonate, united with the matter of the tube, had flowed in two or three places. The shell had shrunk upon itself, so as to stand detached from the sides, and bore very strong marks of fusion. The external surface was quite smooth, and shining like an enamel. The internal part consisted of a mixture of large bubbles and solid parts: the inside of the bubbles had a lustre much superior to that of the outside, and equal to that of glass. The general mass was semi-transparent; but small parts were visible by the lens, which were completely transparent and colourless. In several places this smooth surface had crystallized, so as to present brilliant facettes, steadily shining in certain aspects. I observed one of these

 facettes

facettes on the inside of an air-bubble, in which it interrupted the spherical form as if the little sphere had been pressed inwards at that spot, by the contact of a plane surface. In some chalk near the mouth of the large tube, which lay upon a stratum of silex, another very interesting circumstance occurred. Connected with its lower end, a substance was visible, which had undoubtedly resulted from the union of the carbonate with the silex. This substance was white and semi-transparent, and bore the appearance of chalcedony. The mass of chalk, having attached itself to that above it, had shrunk upwards, leaving an interval between it and the silex, and carrying some of the compound up with it. From thence this last had been in the act of dropping in a viscid state of fusion, as evidently appeared when the specimen was entire, having a stalactite and stalagmite corresponding accurately to each other. Unluckily I broke off the stalactite, but the stalagmite continues entire, in the form of a little cone. This new substance effervesced in acid, but not briskly. I watched its entire solution: a set of light clouds remained undissolved, and probably some jelly was formed; for I observed that a series of air-bubbles remained in the form of the fragment, and moved together without any visible connection; thus seeming to indicate a chemical union between the silex and the carbonate. The shell, fused in the experiment, dissolved entirely in the acid, with violent effervescence.

In the three last experiments, and in several others made at the same time, the carbonate had not been weighed; but no water being introduced to assist the compression, it is probable there was much loss by internal calcination; and owing doubtless to this the carbonates have crumbled almost entirely to dust, while the compounds which they had formed with silex remain entire.

On the 13th of March I made a similar experiment, in which, besides some pounded oyster-shell, I introduced a mixture of chalk, with 10 per cent. of silex intermixed, and ground together in a mortar with water, in a state of cream, and then well dried. The contents of the tube when opened were discharged with such violence that the tube was broken

to

to pieces; but I found a lump of chalk, then in a state of white marble, welded to the compound; which last, in its fracture, showed that irregular black colour, interspersed roughly through a crystalline mass, that belongs to the alpine marbles, particularly to the kind called at Rome *Cipolline.* It was very hard and firm; I think unusually so. It effervesced constantly to the last atom in diluted nitric acid, but much more sluggishly than the marble made of pure chalk. A cloudiness appeared pervading all the liquid. When the effervescence was over, a series of bubbles continued during the whole day in the acid, without any disposition to burst, or rise to the surface. After standing all next day and night, they maintained their station; and the solution, being stirred, was found to be entirely agglutinated into a transparent jelly, breaking with sharp angles. This experiment affords a direct and positive proof of a chemical union having taken place between the carbonate and silex.

VI. *Experiments made in Platina, with Spar, with Shells, and with Carbonate of Lime of undoubted Purity.*

Since I had the honour of laying before this society a short sketch of the foregoing experiments, on the 30th of August last (1804), many chemists and mineralogists of eminence have favoured me with some observations on the subject, and have suggested doubts which I am anxious to remove. It has been suggested, that the fusibility of the carbonates may have been the consequence of a mixture of other substances, either originally existing in the natural carbonate, or added to it by the contact of the porcelain tube.

With regard to the first of these surmises, I beg leave to observe, that, granting this cause of fusion to have been the real one, a material point, perhaps all that is strictly necessary in order to maintain this part of the Huttonian Theory, was nevertheless gained. For, granting that our carbonates were impure, and that their impurity rendered them fusible, still the same is true of almost every natural carbonate; so that our experiments were, in that respect, conformable to nature. And as to the other surmise, it has

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been shown, by comparing together a varied series of experiments, that the mutual action between the lime and the porcelain was occasioned entirely by the presence of the carbonic acid, since, when it was absent, no action of this kind took place. The fusion of our carbonates cannot, therefore, be ascribed to the porcelain.

Being convinced, however, by many observations, that the fusibility of the carbonate did not depend upon impurity, I have exerted myself to remove, by fresh experiments, every doubt that has arisen on the subject. In order to guard against natural impurities, I have applied to such of my friends as have turned their attention to chemical analysis (a branch of the science to which I have never attended) to furnish me with carbonate of lime of undoubted purity. To obviate the contamination arising from the contact of the porcelain tubes, I determined to confine the subject of experiment in some substance which had no disposition to unite with the carbonate. I first tried charcoal, but found it very troublesome, owing to its irregular absorption of water and air.

I then turned my thoughts to the construction of tubes or cups of platina for that purpose. Being unable readily to procure proper solid vessels of this substance, I made use of thin laminated plates, formed into cups. My first method was, to fold the plate exactly as we do blotting paper to form a filter (fig. 26); this produced a cup capable of holding the thinnest liquid; and being covered with a lid, formed of a similar thin plate, bent at the edges, so as to overlap considerably (fig. 28), the carbonate it contained was secured on all sides from the contact of the porcelain tube within which it was placed. Another convenient device likewise occurred: I wrapt a piece of the plate of platina round a cylinder, so as to form a tube, each end of which was closed by a cover like that just described (fig. 27 and 29). (In fig. 26 and 27 these cups are represented upon a large scale, and in 28 and 29, nearly of their actual size). This last construction had the advantage of containing eight or nine grains of carbonate, whereas the other would only hold about a grain and a half. On the other hand, it was not

not fit to retain a thin liquid; but, in most cases, that circumstance was of no consequence; and I foresaw that the carbonates could not thus escape without proving the main point under consideration, namely, their fusion.

The rest of the apparatus was arranged in all respects as formerly described, the same precautions being taken to defend the platina vessel as had been used with the inner tubes of porcelain.

In this manner I have made a number of experiments during this spring and summer, the result of which is highly satisfactory. They prove, in the first place, the propriety of the observations which led to this trial, by showing, that the pure carbonate, thus defended from any contamination, is decidedly more refractory than chalk; since, in many experiments, the chalk has been reduced to a state of marble, while the pure carbonate, confined in the platina vessel, has been but very feebly acted upon, having only acquired the induration of a sandstone.

In other experiments, however, I have been more successful, having obtained some results, worthy, I think, of the attention of this society, and which I shall now submit to their inspection. The specimens are all inclosed, for safety, in glass tubes, and supported on little stands of wax (fig. 31, 32, 33). The specimens have, in general, been removed from the cup or tube of platina in which they were formed, these devices having the advantage of securing both the vessel and its contents, by enabling us to unwrap the folds without violence; whereas in a solid cup or tube it would have been difficult, after the experiment, to avoid the destruction either of the vessel or its contents, or both.

April 16, 1805.—An experiment was made with pure calcareous spar from St. Gothard, remarkably transparent, and having a strong double refraction. A temperature of 40° was applied; but, owing to some accident, the weight was not known. The conical cup came out clean and entire, filled not quite to the brim with a yellowish gray substance, having a shining surface, with longitudinal streaks, as we sometimes see on glass. This surface was here and there interrupted by little white tufts or protuberances disposed

 irregularly.

irregularly. On the ledge of the cup, formed by the ends of the folded platina, were several globular drops like minute pearls, visible to the naked eye, the number of which amounted to sixteen. These seem to have been formed by the entire fusion of what carbonate happened to lie on the ledge, or had been entangled amongst the extremities of the folds, drawing itself together, and uniting in drops, as we see when any substance melts under the blowpipe. This result is preserved entire, without deranging the tube. I am sorry to find that it has begun to fall to decay, in consequence, no doubt, of too great a loss of its carbonic acid. But the globules do not seem as yet to have suffered any injury.

April 25.—The same spar was used with two grains of water, and a heat of 33°. I have reason to suspect, however, that in this, and several other experiments made at this time, the metal into which the cradle was plunged, on first introduction into the barrel, had been too hot, so as to drive off the water. There was a loss of 6·4 per cent. The result lay in the cup without any appearance of frothing or swelling. The surface was of a clean white, but rough, having in one corner a space shining like glass. The cup being unwrapt, the substance was obtained sound and entire: where it had moulded itself on the platina, it had a small degree of lustre, with the irregular semi-transparency of saline marble; when broken, it preserved that character more completely than in any result hitherto obtained, the fracture being very irregular and angular, and shining with facettes in various directions. I much regret that this beautiful specimen no longer exists, having crumbled entirely to pieces, notwithstanding all the care I took to inclose it with glass and wax.

April 26.—An experiment was made with some carbonate of lime, purified by my friend sir George Mackenzie. Two grains of water were introduced, but were lost, I suspect, as in the last case. The heat applied was 32°. The loss of weight was 10·6 per cent. Yet, though made but one day after the last-mentioned specimen, it remains as fresh and entire as at first, and promises to continue unchanged.

changed. The external surface, as seen on removing the lid of the conical cup, was found to shine all over like glass, except round the edges, which were fringed with a series of white and rough sphericles, one set of which advanced, at one spot, near to the centre. The shining surface was composed of planes, which formed obtuse angles together, and had their surface striated, the striæ bearing every appearance of a crystalline arrangement. When freed from the cup, as before, the substance moulded on the platina was found to have assumed a fine pearly surface. Some large air-bubbles appeared, which had adhered to the cup, and were laid open by its removal, whose internal surface had a beautiful lustre, and was full of striæ like the outward surface. The mass is remarkable for semi-transparency, as seen particularly where the air-bubbles diminish its thickness: a small part of the mass being broken at one end, shows an internal saline structure.

April 29.—A cup of platina was filled with several large pieces of a periwinkle* shell, the sharp point of the spiral being made to stand upright in the cup (fig. 30). A heat of 30° was applied, and no water was introduced. The carbonate lost no less than 16 per cent. The shell, particularly the sharp end of the periwinkle, retained its original shape in a great measure, so as to be quite discernible; but the whole was glazed over with a truly vitreous lustre. This glaze covered, at one place, a fragment of the shell which had been originally loose, and had welded the two together. All the angles are rounded by this vitrification; the space between the entire shell and the fragment being filled, and the angles of their meeting rounded, with this shining substance. The colour is a pale blue, contrasted, in the same little glass, with a natural piece of periwinkle, which is of a reddish yellow. One of the fragments had adhered to the lid, and had been converted into a complete drop, of the size of a mustard-seed. It is fixed on the wax (at *b*) along with the other specimens of the experiment (fig. 32). This result shows as yet no sign of decay, notwithstanding so great a loss of weight.

* Turbo terebra *Linn.*

The last experiment was repeated on the same day, and prepared in the same manner, with large fragments of shell, and the point of the periwinkle standing up in the cup. A heat of 34° was applied; a loss took place of 13° per cent. All the original form had disappeared, the carbonate lying in the cup as a complete liquid, with a concave surface, which did not shine, but was studded all over with the white sphericles or tufts, like those seen in the former results, without any space between them. When detached from the cup, the surface moulded on the platina was white and pearly, with a slight gloss. The mass was quite solid, no vestige whatever appearing of the original form of the fragments (fig. 33). A small piece, broken off near the apex of the cone, showed the internal structure to be quite saline. In the act of arranging the specimen on its stand, another piece came off in a new direction, which presented to view the most perfect crystalline arrangement: the shining plane extended across the whole specimen, and was more than the tenth of an inch in all directions. This fracture, likewise, showed the entire internal solidity of the mass. Unfortunately, this specimen has suffered much by the same decay to which all of them are subject which have lost any considerable weight. The part next the outward surface alone remains entire. I have never been able to explain, in a satisfactory manner, this difference of durability, the last-mentioned result having lost more in proportion to its weight than this.

About the beginning of June I received from Mr. Hatchett some pure carbonate of lime, which he was so good as to prepare, with a view to my experiments; and I have been constantly employed with it till within these few days.

My first experiments with this substance were peculiarly unfortunate, and it seemed to be less easily acted upon than any substance of the kind I had tried. Its extreme purity, no doubt, contributed much to this, though another circumstance had likewise had some effect. The powder, owing to a crystallization which had taken place on its precipitation, was very coarse, and little susceptible of close ramming; the particles, therefore, had less advantage than

when

when a fine powder is used in acting upon each other, and I did not choose to run any risk of contamination by reducing the substance to a finer powder. Whatever be the cause, it is certain, that in many experiments in which the chalk was changed to marble, this substance remained in a loose and brittle state, though consisting generally of clear and shining particles. I at last, however, succeeded in obtaining some very good results with this carbonate.

In an experiment made with it on the 18th of June, in a strong heat, I obtained a very firm mass with a saline fracture, moulded in several places on the platina, which was now used in the cylindrical form. On the 23d, in a similar experiment, the barrel failed, and the subject of experiment was found in an entire state of froth, proving its former fluidity.

On the 25th, in a similar experiment, a heat of 64° was applied without any water within the barrel. The platina tube (having been contaminated in a former experiment with some fusible metal) melted, and the carbonate, retaining its cylindrical shape, had fallen through it, so as to touch the piece of porcelain which had been placed next to the platina tube. At the point of contact the two had run together, as a hot iron runs when touched by sulphur. The carbonate itself was very transparent, resembling a piece of snow in the act of melting.

On the 26th of June I made an experiment with this carbonate, which afforded a beautiful result. One grain of water was introduced with great care; yet there was a loss of 6·5 per cent., and the result has fallen to decay. The pyrometer indicated 43°. On the outside of the platina cylinder, and on one of the lids, were seen a set of globules like pearls, as once before obtained, denoting perfect fusion. When the upper lid was removed, the substance was found to have sunk almost out of sight, and had assumed a form not easily described. (I have endeavoured to represent it in fig. 31. by an ideal section of the platina tube and its contents, made through the axis of the cylinder.) The powder, first shrinking upon itself in the act of agglutination, had formed a cylindrical rod, a remnant of which (*abc*) stood up

up in the middle of the tube. By the continued action of heat, the summit of the rod (at *a*) had been rounded in fusion, and the mass being now softened had sunk by its weight, and spread below, so as to mould itself in the tube, and fill its lower part completely (*dfge*). At the same time, the viscid fluid adhering to the sides (at *e* and *d*), while the middle part was sinking, had been in part left behind, and in part drawn out into a thin but tapering shape, united by a curved surface (at *b* and *c*) to the middle rod. When the platina tube was unwrapt, the thin edges (at *e* and *d*) were preserved all round, and in a state of beautiful semi-transparency. (I have attempted to represent the entire specimen, as it stood on its cone of wax, in fig. 34.) The carbonate, where moulded on the platina, had a clean pearly whiteness, with a saline appearance externally, and in the sun shone with facettes. Its surface was interrupted by a few scattered air-bubbles, which had lain against the tube. The intervening substance was unusually compact and hard under the knife. The whole surface (*ebacd*, fig. 31.), and the inside of the air-bubbles, had a vitreous lustre. Thus, every thing denoted a state of viscid fluidity, like that of honey.

These last experiments seem to obviate every doubt that remained with respect to the fusibility of the purest carbonate, without the assistance of any foreign substance.

[To be continued.]

IV. *Upon the Restoration of Sight, which takes place in Mankind and some Animals, without the Assistance of Art. By M.* Portal*.

Sight may be extinguished by various causes, and is restored when these causes are removed; several of them are well known: the absence of the aqueous humour, various overflowings in the anterior and posterior chambers of the eye, indurations of the vitreous humour, paralysis of the optic nerve, some alteration of the crystalline humour, &c. But history has preserved examples of all these blindnesses

* From *Annales du Muséum d'Histoire Naturelle*, vol. vi.

having

having been cured by nature alone. I am about to speak here of the restoration of sight from a cause not hitherto observed, and of which medical men may take advantage.

Twenty years ago, M. Bouhole, a physician at Liege, wrote to me to take charge of a lady of that place affected with a double cataract, and to assist at the operation which was about to be performed upon her by M. Grandjean. This lady was completely blind. I assisted at the operation performed upon her by this celebrated oculist. The crystalline of the right eye was completely and very easily extracted; but the oculist having found some obstacles to the extraction of the left crystalline, he thought of deferring the operation to another time; after having, however, made an anterior incision upon the capsules of the crystalline, as well the capsule which is in common to it with the vitreous humour, as that which is proper to it.

The operation was attended with the happiest success; the lady recovered her sight in her right eye, the one upon which the operation was performed, and she returned home. About two years afterwards, M. Bouhole wrote to us that the lady not only saw extremely well with the eye which had been operated upon, but also that she began to see with the other eye upon which the operation had not been finished: he added, that she perceived a circle of light, the edges of which were progressively enlarged; and that they continued to enlarge more and more, in proportion as the middle of the circle, which was black, diminished; and that she consequently saw better and better.

Grandjean and I believed that this restoration of sight was produced from the edges of the crystalline having regained their pellucid property, and that the more this property was acquired, the sight became more distinct and more directly extended; and, in fact, we hoped that the black substance which the patient always saw before her eye had entirely disappeared. In order, however, to assist the operations of nature, we thought it right to prescribe for the patient every morning for three months some aperients, and among others, four ounces of syrup of cresses and 100 or 200 millepedes bruised while alive. The patient did not make use of these medicines,

medicines, but Nature continued the operation which she had begun, and the lady at last saw objects as clearly and distinctly as any one else. I do not know if the black substance had completely disappeared.

Other facts of the same or an analogous description have been published by oculists; but they always thought, as we did, that it was to a restoration of the pellucidity of the crystalline, in whole or in part, that we ought to attribute the more or less perfect restoration of sight.

Veterinary surgeons and farriers have also informed me, that horses which have lost their sight from a cataract, had recovered it in proportion as this cataract had become less extended. But can it be nothing else than the return of the transparency of the crystalline which occasions the restoration of sight? I think that this is the effect of another cause,—it is the diminution, even the entire destruction of the crystalline which takes place in certain eyes, as well by the consequence of the alteration of the crystalline which had given place to the cataract, as also by various other causes, particular to the crystalline, or common to the various parts of the body.

The crystalline has been often sought after in vain in the eyes of such persons as had died a long time after being couched for the cataract, but no traces of it have ever been discovered: it had been completely destroyed. A human eye, which I dissected a short time ago, was deprived of the crystalline, which had been destroyed perhaps by some morbific cause; at least, I discovered no cicatrix upon the transparent cornea which might afford reason to suppose that it had been ever operated upon. Are there not alterations which hinder the crystalline from nourishing itself sufficiently to retain its volume?

The moment the crystalline is displaced, it becomes like a strange body; it is decomposed, diminished in volume, and destroyed. Abandoned in its residence, after its capsules have been opened by a crucial incision, and having been altered itself in its anterior beds by the needle of the oculist, is it not decomposed, injured, and even *annihilated?* Was it not for this reason that the lady upon whom the

the operation was performed in this manner recovered her sight, having then no longer any obstacle to hinder the rays of light from arriving at the retina?

May it not sometimes happen, that in consequence of a blow, a fall, or other accidents, the crystalline undergoes some alteration, from which the loss of sight soon results, and at last its restoration, when the crystalline is completely destroyed?

The celebrated Scarpa proposed, in place of extracting the crystalline, to destroy it by degrees, by numerous operations: this method has been employed, with equal success, by several of his imitators. But are there not cases in which such operations, although made by the most expert operator, or one of the best anatomists, are rather dangerous than useful? May not these operations augment an ophthalmia when it exists, or even attract it when there is a predisposition in the eye? Would it not be better to desist from such attempts, and to allow Nature, which labours also for the destruction of the crystalline, to complete the work herself, or at least a great part of it?

The remarks which we have made upon the spontaneous destruction of the crystalline prove that Nature, in this case, is not idle, and that she seconds the views of the surgeon, slowly, to be sure, but without any bad consequence.

To conclude:—This destruction of the crystalline produced by Nature is not more surprising than that which she produces upon other parts of the body, and which we shall mention here: for instance, the destruction of the fragments of the pupillary membrane, with which the opening of the iris is closed, and which is torn asunder after birth. What becomes of these membranous fragments? They are decomposed, destroyed, and enter into the circulation like the particles of the disorganized crystalline. The destruction of the entire body of the ossa longa, which takes place on their separation for a length of time, is still more surprising. It is so much so, that in subjects who have died a short time after a supposed formation of such a separation, the substance of the bone was almost entire, while in such as had lived a long time after the disease of the bone, there was only a small fragment

fragment of this same bone in the osseous envelopment, and sometimes none at all.

There are other examples of the destruction of more or less considerable portions of bone. Ruysch remarked in those individuals who had received a fracture of the neck of the thigh bone, and which had not been cured, that the heads remaining in the cotyloidal cavity had so far diminished in size, that they were almost reduced to nothing. I have equally observed in some subjects in whom the rotula was broken transversely, that the portion of the bone attached to the ligament of the tibia was diminished in size in a singular manner, as well as that attached to the extremities of the extensor muscles of the leg, but the latter in a proportionably less degree. In all these cases, it is for want of nourishment that the parts decrease, and this nourishment diminishes or ceases as soon as the circulation of the nutritive matter slackens or is interrupted. And it is this which takes place when the lymphatics, or blood-vessels, or the nerves, are compressed or destroyed. May we not ascribe to this cause the diminution which takes place, after birth, of the liver in general, and of the left lobe in particular? The diminution happens from this; that the blood, which was conducted there by the hepatic artery and by the portatory and umbilical veins, is no longer removed by the latter vein, and which was principally distributed through the left lobe. Anatomists have remarked, that in the fœtus, the super-renal bodies and their blood-vessels were very great relatively to the kidneys and the vessels, but that after birth the vessels of the super-renal bodies diminish, and those of the kidneys grow large in proportion: this occasions the diminution in volume, and even the *annihilation* of the former parts and the increase of the others. I do not doubt that the thymus is effaced in a similar manner. It is certain that it loses its volume in proportion as its arteries contract; and is it not because the lungs receive incomparably more blood after birth than before, that the vessels of the thymus, with which the pulmonary vessels are connected, receive less blood also, and therefore at last disappear?

There is another example of a no less remarkable destruc-

tion,

tion, which we may mention here; and that is, the destruction of a division or separation, which is common to the two ossa longa of the leg of the fœtus of all the cloven-footed animals, the *sus* genus only excepted. These two bones, which in the fœtus are very well separated, unite together after birth; about the fourth week they are joined together by their sides, which had been only contiguous; and about the fifth month, the common division, which separated their two cylindrical cavities, entirely disappears, in such a manner, that of the two bones one only remains, which is vulgarly called the *cannon*.

This was very distinctly remarked by Fougeroux *, without his being able to ascertain the cause, notwithstanding the ingenious experiments made by that philosopher to discover it.

Thus it is evident that Nature is as wonderful for the manner in which she effects the destruction of different parts, as for the great use which she derives from it, as well in the perfection of our organs as in the cure of our diseases.

V. *Analysis of the Iron Ore described by several Mineralogists under the Denomination of Spathic Iron Ore, White Ore of Iron, Spathic Iron, Ferriferous Carbonated Lime with Manganese, &c., Ore of Steel, &c.* By J. J. Drappier, *Lecturer on Chemistry in the Polytechnic School. Read in the National Institute of France* †.

Of the three specimens which were submitted to a chemical analysis, two of them formed a part of the beautiful collection of the Council of Mines ‡. They were sent to me by M. Daubisson, who very properly added the following description.

* Acad. de Sciences 1772.

† From *Annales de Chimie*, tome lvi.

‡ The specimens from which the fragments analysed were detached are numbered in the above collection $\frac{565}{15}$ (being that of Baigory) and $\frac{797}{87}$ (being that of Vaunaveys).

No. I.

No. I. *Spathic Iron of Baigory.*

Contexture.—The mass partly in small grains and almost compact, partly in large grains, very much laminated, each grain presenting distinctly a rhomboidal figure, and being half an inch in size. Its surface is bristled with large lenticular crystals nearly an inch high, which are very obtuse rhomboids, and a little deformed (the *equiaxis* variety of M. Haüy). These crystals form a continued mass with the part not crystallized, and are evidently the same substance.

Colour.—Grayish white; in some places of a deep dirty flesh red. The compact parts are the whitest. The surface, particularly that of the crystals, is of a brownish yellow, having sometimes a slight red reflection.

N. B. The latter colour appears to be the effect of the contact of the atmospheric air; the alteration does not appear to penetrate into the mass.

Transparency.—Very translucid upon the edges.

Hardness.—A little harder than the calcareous spar. The compact part is, besides, of considerable consistency; it is fresh, and by no means altered.

Gravity—3·83.

This specimen appeared pure and homogeneous; we saw, however, in it some grains of quartz and pyrites, but in small quantity, and very small in size. A fragment of this specimen, tried in the forge, yielded a fine button weighing 40 per cent. It was brought from Ustelegny, near Baigory, in the department of the Lower Pyrenees.

No. II. *Spathic Iron of Vaunaveys.*

This specimen differs from the preceding in so far as it does not present distinct crystals, although the whole mass is crystalline in large grains, presenting the rhomboidal form very distinctly. It is not so white in colour; it is of a deep brownish yellow. It has little lustre or transparency; it is not so hard as the other, and its consistence is less. Its gravity is 3·60. It was brought from Vaunaveys, three leagues south-east from Grenoble, and nine leagues south-south-west from Alevard, in the department of Isère.

It

It is necessary to remark that this specimen, among these rhomboidal grains, contained some grains of carbonate of lime, which were white, and very distinct from those of the spathic iron: there were also some veins and crystals of quartz and some atoms of pyrites in the above specimen.

No. 3. *Spathic Iron of Allevard, Department of Isère.*

This specimen was transmitted to me by professor Hassenfratz. It very much resembled No. 2, in colour, lustre, and transparency; it is well crystallized, very homogeneous, and in good preservation. It is easily divided into rhomboids, the plates of which, when they are slender, are perfectly diaphanous.

Analysis.

(A) I first endeavoured to ascertain if the three specimens of spathic iron contained any lime. In order that I might not confound that which might be in the matrix with that which formed part of the crystals, I carefully separated the particles which differed in appearance from the dominant part; I also took care only to employ crystalline fragments. After having reduced them into impalpable powder, I introduced five grammes of each of the specimens into three glass phials, numbered. I added a little distilled water in order to dilute the powder and to diminish the action of the sulphuric acid, which I poured in by degrees to complete saturation. As in the cold the action was almost nothing, the three phials were exposed to a heat scarcely capable of bringing the mixture to the boiling point; the combination of the sulphuric acid then slowly commenced; it was accompanied with the effervescence usual in the disengagement of carbonic acid gas until the solution was complete. No. I. yielded no residue; No. II. yielded what scarcely weighed 0·01 gramm.; that of No. III. the only one which could be ascertained, weighed 0·04 gramm. It was nothing else than a little silex, which had probably escaped observation from being in the crystals. The solution was preserved a long time, and remained colourless, and deposited nothing. Had these substances contained

tained ever so little lime*, sulphate of lime would have been infallibly precipitated, particularly after cooling, because the water in the solution was but little; and that there might not be an excess of acid, I had the precaution not to add any new sulphuric acid before that previously poured in had entered into combination: besides, in a preliminary trial which I made by means of the muriatic acid, I obtained no precipitate by the oxalate of ammonia.

(B) In order to separate the oxide of manganese which I supposed to be contained in the spathic iron, I thought the following process the most proper:

I diluted with distilled water five grammes of the same substance. I poured nitric acid on it by little and little, with the same precautions as above detailed. The solution took place very rapidly; the same phænomena were again produced, but with a disengagement of nitrous vapours. The solution was afterwards decanted into a porcelain cup, evaporated to dryness, and diluted in a new quantity of water. I repeated these operations twice successively, because I had remarked that once was not sufficient, the oxide of iron remaining suspended in the liquor and passing through all the filters. I would even advise those who wish to repeat these processes, previously to imbibe the filtering paper with a little distilled water; the oxide of iron will not then pass through so easily, and will also be more easily separated. If it should happen that the oxide is so much incrusted that it is not possible to detach it without a great loss, the best course to follow would be to incinerate the filter in a crucible of silver or platina. By following this method I extracted the following quantities of the red oxide of iron: from No. I. 3·05 grammes; from No. II. 2·45; from No. III. 2·45 grammes.

(C) The liquors which had passed through the filters

* More than a year ago, I had occasion to analyse, in the laboratory of the Council of Mines, a specimen of white ore of iron, which was sent to me by M. Lenoir, engineer of the mines. This specimen, which had the appearance of some of the stones for building (limestones), found in the neighbourhood of Paris, did not yield a single atom of lime.

were

were perfectly colourless: after having concentrated them, I precipitated from them, by carbonate of soda, a white flaky matter, very light, which, at first sight, I took for the oxide of manganese.

It having appeared, upon pouring a little caustic soda into the liquors, that in spite of the excess of alkali a portion of white matter remained in solution, I boiled them in order to drive off the carbonic acid, which it is believed has the property of dissolving several earthy and metallic substances. I filtered them afterwards in order to collect the precipitates: after having cleaned them well, I separated them from the filter, and calcined them for fifteen minutes in a crucible of platina: No. I. weighed 0·25 grammes; No. II. 0·70; No. III. 0·68.

(D) The gelatinous appearance of these precipitates surprised me so much the more, as I did not know this property in the carbonate of manganese: I was still more astonished when I took them out of the crucible as white after calcination as they were before; because the carbonate of manganese loses its acid and becomes black at a degree of heat even inferior to that which I had used, while the matter of which we are now speaking had retained a good deal of carbonic acid and preserved all its whiteness. A second calcination did not produce any remarkable change. This substance melted by the blowpipe with glass of borax, dissolved with effervescence without being coloured: nitrate of potash thrown upon the burning mass did not produce the violet colour which characterizes the oxide of manganese, notwithstanding I made use of a supporter of baked clay in place of charcoal, in order to remove every thing which could obstruct the colour. The precipitates obtained from the salts of manganese by the caustic alkalis speedily become brown in the air; the above substance presented no such appearance: dissolved in the sulphuric and nitric acids, it was neither precipitated by the hydro-sulphurets nor by the alkaline prussiates: the latter scarcely give it a blueish cast. By means of the oxalate of ammonia a slight precipitate is obtained, which is slowly formed, and which is redissolved in water or in an excess of acid. I ought to observe here,

 that

that it is right to be on our guard against a circumstance which may impose upon us: frequently, after a certain number of precipitations and filtrations, the re-agents indicate the presence of lime, in a small quantity to be sure, in the acid liquors which formerly appeared to contain none at all. This lime generally proceeds from a little chalk which some filtering papers contain, as is proved by the effervescence which takes place when these liquors are filtered.

(E) The properties here explained of the unknown substance appear to me to characterize sufficiently one of the earths to direct my attention to that quarter. Magnesia was the earth which seemed to have the greatest resemblance to the above substance; in fact, both were equally well dissolved by sulphuric acid; with the latter a bitter salt was formed as well as with the former. It is not precipitated from its solutions in the acids (when the latter are properly diluted with water) by means of the carbonates of soda and ammonia; with caustic alkalis it yields a precipitate insoluble in an excess of alkali, but very soluble in the carbonate of ammonia. One property alone, that of retaining carbonic acid, even after a very strong calcination, seems to exclude all parallel of this substance with magnesia; because, according to the generality of chemists, nothing is easier than to deprive the carbonate of magnesia of its acid by means of calcination. But I had often occasion to experience that this result is not so easy to obtain as is generally imagined; the identity of the above substance with magnesia, then, appears to me sufficiently established not to regard it as a new earth.

(F) In order to complete this analysis, it still remained to know whether the three minerals of spathic iron contained water and carbonic acids. For this purpose, I calcined five grammes of each in a crucible of platina. No. I. lost by this operation 1·70 grammes, No. II. lost 1·85, No. III. lost 1·83: these numbers, it is true, could not represent the amount of water and carbonic acid: the loss in weight, occasioned by their disengagemet, was diminished by the hyper-oxygenation of the iron. In fact, the nitrous gas, produced by these minerals when they are dissolved in the

3 nitric

nitric acid, and the green precipitates obtained from their solutions in the sulphuric and muriatic acids, prove evidently that the iron they contain is very little oxygenated. But as I collected all the products of the analysis in the state in which they are found in the minerals after calcination, it follows, that the results ought to be similar. To conclude, it is easy to make the necessary correction, by admitting, with M. Proust *, that iron, in order to oxidate itself at the *minimum*, augments 0·28 in weight, and 0·48 in order to oxidate itself at the *maximum*, without, however, deciding any thing upon the question of the intermediate oxidations. Upon this hypothesis the proportions will be established as follows:

Real weight in grammes of the products obtained by the foregoing analyses.

	No. I.	No. II.	No. III.
Red oxide of iron, mixed with a very small quantity of oxide of manganese. See (G). - -	3·05	2·45	2·45
Magnesia - - -	0·25	0·70	0·68
Loss by calcination - -	1·70	1·85	1·83
Silex - - - -	0·00	0·00	0·04
Total grammes	5·00	5·00	5·00

In order to estimate these products by centesimal parts, it is only necessary to multiply the above numbers by 20; we shall then have the following results:

	No. I.	No. II.	No. III.
Red oxide of iron - -	61·0	49·0	49·0
Magnesia - - -	5·0	14·0	13·6
Loss by calcination - -	34·0	37·0	36·6
Silex - - - -	00·0	00·0	00·8
Total	100·0	100·0	100·0

The following table presents the proportions of the substances contained in the three specimens of spathic iron, calculated on the principle that the iron in them was oxidated at the *minimum*, according to the proportion, given by

* Journal de Physique, tom. lix.

by M. Proust, of 148 for the maximum and 128 for the minimum of oxidation, the metal being represented by 100:

	No. I.	No. II.	No. III.
Oxide of iron at the minimum	52·75	42·38	42·38
Magnesia - - -	5·00	14·00	13·60
Water and carbonic acid -	42·25	43·62	43·22
Silex - - - -	00·00	00·00	00·80
Total	100·00	100·00	100·00

Upon this hypothesis a quintal of the mineral No. I. contains in metallic iron 41 per cent., No. II. 33 per cent., and No. III., calculated upon the same hypothesis, contains 33 per cent.

This last result from No. I. agrees very well with that of the experiment made at the forge of the laboratory of the Council of Mines. As to the rest, I do not know if they have been tried.

(G) The celebrated Bergman, in a memoir entitled *De Mineris ferri albis* *, announced that he found a very great quantity of manganese in spathic iron. The authority of the above chemist and of his followers was sufficient to induce me to inquire if the oxide of this metal could not have been kept in combination by the oxide of iron. In order, in the first place, to ascertain its existence, I melted the ore by the blowpipe with glass of borax, as well in its natural state as after being calcined, without obtaining, even with the assistance of nitre, any colour which announced the presence of manganese. I perceived, indeed, marks of this metal on melting five grammes of each mineral with three parts of caustic potash; but the green tint which the alkali took was very feeble; it quickly disappeared, and the oxide of manganese which I extracted was not appreciable in the balance.

It is probable, then, that Bergman mistook the magnesia for the oxide of manganese. We may even ascertain that the presence of this metal in spathic iron is by no means established by the facts quoted in his dissertation upon the

* Opuscula Physica et Chemica, vol. ii.

white

white ores of iron*; and, notwithstanding the author has described at such length the properties of manganese, we may say with M. Fourcroy†, "that one would be led to believe, on reading it with attention, that it was written by its author rather to furnish him with an opportunity of speaking of this last metal under the name of *white ore of iron*, than to detail the properties of carbonate of iron."

By even supposing that lime‡ and manganese§ enter into the composition of spathic iron in any notable quantity, we ought to conclude, from all the facts above stated, that the denomination of *carbonated ferriferous lime with manganese*, cannot apply to all the white ores of iron. But if the identity in the form of carbonated lime, with the specimens which are the subject of this memoir, is well established, how can we conclude that mineralogical characteristics, drawn from external form, are sufficient for determining the species?

VI. *Experiments on the Growth of White Thorn, pointing out a better Method of propagating that valuable Plant than had before been practised. By* Samuel Taylor, *Esq. of Moston, near Manchester*||.

For these experiments the silver medal of the Society for the Encouragement of Arts, &c. was voted to Mr. Taylor, from

* It is remarkable enough that it was precisely in this dissertation that Bergman substituted the expression of *magnesium* to designate *manganese* for that of *magnesia*, to which the epithet of *nigra* is added, in order that the metal might not be confounded with the substance known by the name of magnesia alba.

"Ne—vel cum magnesia alba confundatur, terminationem neutralem, nominibus metallorum omnium, exceptâ platinâ communem addidimus."

† Systeme de Connoissances Chimiques.

‡ It is not astonishing that several chemists have found lime, since the matrix of certain ores of spathic iron visibly contains a very great quantity of it. Bergman says, in speaking of this earth,—"Pondus calcis multùm variat; in quibusdam paucas centesimas reperimus. In spathosis circiter decima pars ea constat, in aliis dimidium, nunquam verò penitus defuit."

§ As to manganese, it is so much the more interesting to ascertain if these ores contain any of it, because the generality of metallurgists attribute to this metal the property of producing natural steel.

|| From *Transactions of the Society of Arts*, &c. 1805.

from whom the following accounts were received. Specimens of the plants are reserved in the repository of the society, where they may be seen by agriculturists.

GENTLEMEN,

Every one of you, I think, will allow that fences are material objects to be attended to in agriculture; you must also be convinced that there is no plant in this kingdom of which they can so properly be made as the Cratægus oxyacantha Linnæi, or common White Thorn. In consequence of my being convinced of this, I have been induced to make a few experiments to effect the better propagation of that valuable plant; the result of which, along with specimens of my success, I beg leave to submit to your inspection.

In the year 1801, I had occasion to purchase a quantity of thorns, and finding them very dear, I was determined to try some experiments, in order if possible to raise them at a less expense. I tried to propagate them from cuttings of the branches, but with little or no success. I likewise tried if pieces of the root would grow; and I cut from the thorns which I had purchased about a dozen of such roots as pleased me, and planted them in a border along with those I had bought. To my great astonishment, not one of them died; and in two years they became as good thorns as the average of those I had purchased. The thorns I purchased were three years old when I got them. In April 1802, I had occasion to move a fence, from which I procured as many roots of thorns as made me upwards of two thousand cuttings, of which I did not lose five in the hundred.

In the spring of 1803, I likewise planted as many cuttings of thorn roots as I could get. In 1804, I did the same; and this year I shall plant many thousands.

I have sent for your inspection specimens of the produce of 1802, 1803, and 1804, raised after my method, with the best I could get of those raised from haws in the common way, which generally lie one year in the ground before they vegetate. They are all exactly one, two, and three years old, from the day they were planted.—I was so pleased with my success in raising so valuable an article to the farming interest of this kingdom, at so trifling an expense, (for it is merely

merely that of cutting the roots into lengths and planting them), that I was determined to make it known to the world, and could think of no better method than communicating it to your society; and should you so far approve of this method of raising thorns, as to think me entitled to any honorary reward, I shall receive it with gratitude, but shall feel myself amply repaid for any trouble I have been at, should you think it worthy a place in the next volume of your Transactions.

The method of raising the thorns from roots of the plant, is as follows.

I would advise every farmer to purchase a hundred or a thousand thorns, according to the size of his farm, and plant them in his orchard or garden, and when they have attained the thickness of my three-year-old specimens, which is the size I always prefer for planting in fences, let him take them up and prune the roots in the manner I have pruned the specimen sent you, from which he will upon an average get ten or twelve cuttings from each plant, which is as good as thorns of the same thickness; so that you will easily perceive that in three years he will have a succession of plants fit for use, which he may if he pleases increase tenfold every time he takes them up.

The spring (say in all April) is the best time to plant the cuttings, which must be done in rows half a yard asunder, and about four inches from each other in the row; they ought to be about four inches long, and planted with the top one-fourth of an inch out of the ground, and well fastened; otherwise they will not succeed so well.

The reason why I prefer spring to autumn for planting the roots, is, that were they to be planted in autumn, they would not have got sufficient hold of the ground before the frost set in, which would raise them all from the ground; and, if not entirely destroy the plants, would oblige the farmer to plant them afresh.

I have attached the produce of my three-year-old specimen to the plants it came from, cut in the way I always practise; on the thick end of the root I make two, and on the

the other end one cut, by which means the proper end to be planted uppermost, which is the thick one, may easily be known.

Although I recommend the roots to be planted in April, yet the farmer may, where he pleases, take up the thorns he may want, and put the roots he has pruned off into sand or mould, where they will keep until he has leisure to cut them into proper lengths for planting; he will likewise keep them in the same way until planted.

The great advantage of my plan is: first, that in case any one has raised from haws a thorn with remarkably large prickles, of viggrous growth, or possessing any other qualification requisite to make a good fence, he may propagate it far better and sooner, from roots, than any other way. Secondly, in three years he may raise from roots a better plant than can in six years be raised from haws, and with double the quantity of roots; my three-year-old specimen would have been half as big again, had I not been obliged to move all my cuttings the second year after they were planted.

It would not be a bad way, in order to get roots, to plant a hedge in any convenient place, and on each side trench the ground two yards wide, and two grafts deep; from which, every two or three years, a large quantity of roots might be obtained, by trenching the ground over again, and cutting away what roots were found, which would all be young and of a proper thickness. I do not like them of a larger size than the specimens sent.

I am at present engaged in several experiments, to endeavour to propagate the thorn from the branches, which, if successful, I will communicate to you; but I am of opinion, that what is now done is sufficient.

Should the society require any further explanations, I shall be happy in doing my utmost to furnish such explanations.

Samuel Taylor.

Moston, near Manchester,
May 6th, 1805.

To the Society of Arts, &c.

A List

A List of the Specimens sent.

No. 1. consists of four specimens of roots, which were planted the latter end of April, 1804; two with only one shoot a-piece, and two with several.

No. 2. consists of two specimens of the best one-year-old seedlings I could select out of 40,000 I purchased this spring, which in fact are two years old from the time the haws were gathered; for they generally remain one year in the ground before they vegetate.

No. 3. consists of two specimens of roots, planted the latter end of April, 1803, which have not been moved before since they were first planted; one with only one shoot, and the other with several.

No. 4. consists of the best two-year-old seedlings which I have been able to procure.

No. 5. consists of two specimens of roots, planted the latter end of April, 1802, which I was obliged to remove the spring after I first planted them; this of course retarded their growth, by having fresh roots to make. These specimens are pruned in the way I always prune thorns when I plant them in my fences; the roots, you may see, are cut, and the top I should have cut at the mark you will find about four or five inches above the root, and shortened the branches below that mark, to about three inches from the stem. I have attached to each specimen the roots I pruned from it, cut into such lengths as I should have done had I used the thorns myself. I think one produced sixteen and the other twenty-two; but that you will be able to judge of when you see them.

No. 6. consists of the best three-year-old seedlings I could get; but both No. 4. and No. 6. labour under the same inconvenience as No. 2, from the haws remaining in the ground generally one year before they vegetate.

VII. *On*

VII. *On the Stratification of England; the intended Thames Archways, &c.* By JOHN FAREY, *Esq.*

To Mr. Tilloch.

SIR,

A REGARD for the interests of science has long occasioned me and others to regret, that a truly valuable and extensive body of facts on the stratification of England, Wales, and part of Scotland, the labour of 14 years or more, spent in the most intense application to the subject by a gentleman of Bath (Mr. *William Smith*, engineer, now resident in Buckingham street, Strand, London), should remain useless on his hands, for want of sufficient public encouragement to induce him to publish his Maps, Sections, Drawings and Descriptions of extraneous Fossils and other particulars, most of which are new, and appear quite essential to the placing of the Geology of our own country, and perhaps of the whole terraqueous Globe, among the accurate sciences. Mr. Smith has traced the out-crop of a stratum of Chalk, that is, its appearance on the surface of the land, or rather in the hangings of certain ranges of our English hills for near 700 miles in length! The exact uniformity of appearances of this out-crop in innumerable instances, corroborated by the sinking of wells and other works of art upon or in this stratum, in the greatest variety of situations, has fully warranted the conclusions; that this immense mass of matter consists of a vast number of layers of chalk and siliceous substances, the latter in almost every variety of form, from that of solid black flint, to a white and fine sand or grit-stone; the whole of which layers (except of flints) have in general so much the appearance of *chalk* for 400 feet or more in thickness, as hitherto to have passed under that denomination, and which it may perhaps be well still to retain as a general name for this assemblage of strata. In like manner the out-crop has been traced of an immense succession of argillaceous, siliceous and ferruginous matters lying upon the above chalk stratum, wherever the same is found to have any regular strata upon it in England; which strata Mr. Smith

Mr. Smith may be well entitled to denominate the *London clay*, from the circumstance of the metropolis and its environs (where alluvial gravel, sand, &c. or peat do not intervene) standing immediately upon a very considerable and remarkable red clay layer or stratum, forming part of this uppermost assemblage of the British strata. It is not my present intention, nor would it be proper towards my friend Mr. Smith, to attempt, were I able, completely to develop the theory which the immense body of facts in his possession (and numerous others in my own, collected before and since 1801, when I first became acquainted with Mr. Smith) go to establish: suffice it to say, that the most complete and certain rules have been, or may in every instance be, deduced for ascertaining the relative position (which probably never varies) of each distinct stratum, however thin, with regard to those above and below it in the series (or *natural order of the strata*, as Mr. Smith called it in his first printed prospectus); rules equally general have, or will on sufficient inquiry, be found, for identifying each particular stratum, either by the knowledge of its relative position with other known strata in its vicinity, by the peculiar organized remains imbedded in it, and not to be found in the adjoining strata, or by the peculiar nature and properties of the matter composing the stratum itself. By a reference to the rules above alluded to, and the consideration of other well-established and unvarying particulars, the *alluvial* matters can be certainly distinguished; by which term are here meant, the fragments of the regular strata, more or less mixed with each other, or with extraneous matters, and rounded or worn, lying upon the regular strata (for such are rarely or probably never seen in or under the strata) and are there found deposited, apparently by the action of violent currents of water (assisted perhaps by the general principle of gravity, in circumstances which have never yet been contemplated among the physical inquiries of mathematicians), the manner of these alluvial deposits being perfectly different from, and apparently regulated by laws quite dissimilar from those which obtained when the deposition of the strata took place. A further remark it is

essential

essential to make, and which relates to the truly enormous and violent breaking up, which the strata have almost universally undergone (probably owing to their gravity under the circumstances above alluded to), the effects of which are well known to miners, and to all others who extensively open the soil in directions nearly horizontal, by the name of faults, troubles, dykes, fissures, &c. &c.

I was led, Sir, on the present occasion, to make the above remarks, from having lately, for the sake of information, visited the works going on near the Horse Ferry, in Rotherhithe, (about $2\frac{3}{4}$ miles below London Bridge) for making an archway under the Thames river, intended for the passage of horses and carriages, in case, on executing the perpendicular shaft, now in hand, and a drift or drain therefrom under the river, it shall be judged practicable at this place to tunnel and form an archway under its bed, upon a sufficiently high level for carriages to descend down into it, and ascend again at the opposite end, by sloping roads of easy and convenient lengths. Should the result of the present experiment appear against a carriage archway at the Horse Ferry, it is still hoped by Mr. *Robert Vazie*, the projector and superintendant of the works, that a smaller arch may be here formed under the bed of the river, at a depth not inconvenient to foot-passengers, who are to descend to the same by a circular or well staircase at one end, and ascend by the like means from the other end. Mr. Vazie, with a liberality highly creditable to himself and to those who employ him, showed me the plans and sections of the proposed works, and of the borings which he made, together with specimens of some of the matters obtained thereby, at different depths on each side of the river at this place. These were denominated as follows: viz.

On the South Side.		On the North side.	
Feet.			
6	From high water level to the surface of the ground.		
9	Brown clay.	Feet.	(Below high-water mark.)
21	Gravel.	12	Gravel.
19	Strong blue clay.	33	Strong blue clay.
55		45	
		8	Chalk.

On the South Side.		On the North Side.	
Feet.		Feet.	
55	(Brought over)	45	
8	Chalk.	7	Light blue and brown stratum resembling sand.
4	Concreted rock.	3	Sand.
16	Green dry sand.	4	Variegated blue and brown hard clay.
3	In firm gray wet sand.	2	In wet sand.
86		61	

The river in this place being 772½ feet wide at high water of spring tides, and then 38 feet 10 inches deep in the middle.

I have already, sir, declared myself convinced* of the practicability and usefulness of forming archways under Rivers in almost any situations, and it is principally from a desire to contribute my mite towards promoting this highly useful mode of communication that I have troubled you herewith. The shaft or well at present sinking in Rotherhithe, is, as Mr. Vazie informs me, intended, in the first instance, for ascertaining, more fully than boring can do, the nature and condition (as to springs of water) of the strata or alluvial matters under the south bank of the river; and, during the progress thereof, I beg to call the attention of Mr. Vazie and those concerned, to the circumstance of preserving frequent and ample specimens of all the matters sunk through, with reference to the exact depth and thickness of each, and the state of the water, as ascertained by the strokes of the engine-pump from time to time. I beg further to mention, that, as far as my recollection of Mr. Smith's map now serves me, the top of the great Chalk stratum above mentioned, is to be found on the surface at no great distance south of the head of the Croydon canal, about ten miles from London: also, that at the foot of Ridge Hill, 16½ miles from London, on the road to St. Albans, the same stratum appears again; the surface between these two places (passing through the metropolis) being occupied by the whole or the lowermost of the strata

* Dr. Rees's Cyclopædia, article *Canal*, sect. *Thames River*, &c.

comprehended in the London clay. I have mentioned the above two points, one sufficiently near in direction to the south and the other to the north of the proposed archway, as a guide to the inquiries which I am going to suggest; but in all probability some other line, nearly north and south, (particularly on the north side of London,) may be found more eligible, in which I beg to recommend that it should be minutely inquired by means of the many different Wells which have been sunk, by the recent cuttings of the Croydon Canal and other excavations, as well as by the cropping of the several strata which compose the London clay, both at its southern and northern edge, what are the nature, the actual succession, and thicknesses of the several strata, which may be expected to exist under the vale of the Thames at London, after the alluvial matters therein have all been sunk through, and the strata reached. During the last autumn, I, and others, had an opportunity of making part of the above inquiries on the line of the Croydon canal, and am enabled to state, that, proceeding northward from Sydenham town to near Brockley-Green, the canal will be found all the way cut in a thick stratum of very strong red clay, whose characteristic mark seems to be, two or more remarkable layers of *ludus Helmontii*, or clay-balls; which clay-ball stratum forms the surface in most parts near London that I have examined, except where either gravel or alluvial matters exist thereon, or where an excavation or abrasion of the surface has taken place, and exposed strata which are to be found lower in the series. In going southward from Brockley-Green, and descending from the surface of the stratum above described (for the course of one mile and a half, or rather less,) to the foot of Plough-garlick Hill, near the London and Greenwich road, a great number of the upper strata of the London clay may be traced in succession, and ample specimens of each may be obtained from the new banks of the canal. It may be necessary here to mention, that the exposure of the edges of the strata, in the cutting of the canal between Brockley and the Greenwich road, and in numerous other places on the southern side of the Thames vale, seems owing to an enormous dislocation or lift of the strata in those places, and to an amazing abrasion or wearing away of the edges of the uppermost

uppermost of those strata. The dislocations above mentioned present to naturalists a most favourable opportunity, by ascending from the several chalk-pits in Greenwich, Chalton, Woolwich, &c. towards the top of Shooter's Hill, of distinctly tracing, as I apprehend, every stratum in the London clay. I have ventured to suggest these, as among the numerous methods which might be taken to ascertain, whether regular strata have already or may hereafter be reached by the borings or sinkings for the Rotherhithe archway: justice, perhaps, requires of me to say, in this place, that the borings above given, and specimens shown to me by Mr. Vazie, present to me none of the characters of the undisturbed London strata, but decidedly those of alluvial matters; and which matters, I have too much reason to fear, extend to still vastly greater depths at Rotherhithe. I have hinted elsewhere, that had the regular or clay-ball stratum appeared at the surface on each bank of the river, still, before an expensive tunneling is undertaken, it would be prudent to bore at short intervals across the river sufficiently far into the clay to ascertain whether any fissures filled with alluvial matters exist therein under the river, which it may be extremely difficult if not impossible to work through. It has long appeared probable to me, that the vale of the Thames, from Charing-Cross to the Tower on the north side, and from the foot of Camberwell Hill to the foot of Plough-Garlick Hill on the south, has been occasioned by two fissures, one along each of those lines or nearly, and that the whole of the strata between those lines are sunk down, perhaps, to a depth far below practicable boring or well-sinking, and that this chasm (a similar and remarkable instance of which I have traced at Woburn, in Bedfordshire,) has been filled up, almost to its present level, by successive alluvial deposits of sand, clay, and other matters washed from the neighbouring eminences; and that, in after and more quiet periods, the growth of peat, and the gradual subsidence of mud, have completed the filling up of the valley to its present state. Having far exceeded the limits which I proposed to myself when I began this letter, I beg to conclude, and am,

Yours, &c.

12, Upper Crown-street, Westminster,
21st May, 1806.

JOHN FAREY.

VIII. *Method of manufacturing cheap and durable Paints with Fish Oil. By Mr.* Thomas Vanherman, *of Mary-le-bone-street, Golden-square**.

In the session 1805, the Society for the Encouragement of Arts, Manufactures, and Commerce, voted its silver medal and twenty guineas to Mr. Vanherman for the following communication:

Gentlemen,

Having applied a great portion of my time, for several years past, to discover a method of preparing a cheap and durable composition for the defence and preservation of all work exposed to the inclemency of the weather, I have now the satisfaction of laying before the Society for the Encouragement of Arts, &c. specimens of some of the above colours ready prepared for use, which will, I flatter myself, be found superior to all others for cheapness and durability, equal to any in beauty, and not subject to blister or peel off by the sun.

The vehicle made use of for the said paints is fish oil, the preparation of which is so simple, that, when known, gentlemen who have large concerns to paint may have this composition of any colour manufactured, and laid on by their labourers. I have sent a bottle of the prepared oil; also a number of patterns of various colours. The highest price of any does not exceed threepence per pound, and many of them so low as twopence, in a state fit for use. I have likewise sent a pot of white lead which has been ground with prepared fish oil, and which, when thinned with linseed oil, surpasses any white hitherto made use of, for resisting all weathers and retaining its whiteness. I hope my humble endeavours will merit the approbation of the society, before whom I will, at any time they shall please to appoint, make the various experiments they may require.

Relying on your encouragement, I am, gentlemen, with due respect,

Your most obedient humble servant,

Thomas Vanherman.

* From *Transactions of the Society of Arts*, &c. 1805.

To

To refine one Ton of Cod, Whale, or Seal Oil for Painting, with the Cost attending it.

	£.	s.	d.
One ton of fish oil, or 252 gallons -	36	0	0
32 gallons of vinegar, at 2*s.* per gallon	3	4	0
12 lbs. litharge, at 5*d.* per lb. - -	0	5	0
12 lbs. white copperas, at 6*d.* ditto - -	0	6	0
12 gallons of linseed oil, at 4*s.* 6*d.* per gallon	2	14	0
2 gallons of spirit of turpentine, at 8*s.* ditto	0	16	0
	43	5	0

252 gallons of fish oil,
12 ditto linseed oil,
2 ditto spirits of turpentine,
32 ditto vinegar,

298 gallons, worth 4*s.* 6*d.* per gallon.

	£.	s.	d.	
Which produces -	67	1	0	
Deduct the expense -	43	5	0	
	23	16	0	profit.

To prepare the Vinegar for the Oil.

Into a cask which will contain about forty gallons put thirty-two gallons of good common vinegar; add to this twelve pounds of litharge, and twelve pounds of white copperas in powder; bung up the vessel, and shake and roll it well twice a day for a week, when it will be fit to put into a ton of whale, cod, or seal oil, (but the southern whale oil is to be preferred, on account of its good colour, and little or no smell;) shake and mix all together, when it may settle until the next day; then pour off the clear, which will be about seven-eighths of the whole. To this clear part add twelve gallons of linseed oil, and two gallons of spirits of turpentine; shake them well together; and after the whole has settled two or three days it will be fit to grind white lead and all fine colours in; and, when ground, cannot be distinguished from those ground in linseed oil, unless by the superiority of its colour.

If the oil is wanted only for coarse purposes, the linseed oil and oil of turpentine may be added at the same time that the prepared vinegar is put in, and, after being well shaken up, is fit for immediate use without being suffered to settle.

The vinegar is to dissolve the litharge, and the copperas accelerates the dissolution, and strengthens the drying quality.

The residue, or bottom, when settled, by the addition of half its quantity of fresh lime water, forms an excellent oil for mixing with all the coarse paints for preserving outside work.

Note. All colours ground in the above oil, and used for inside work, must be thinned with linseed oil and oil of turpentine.

*** The oil mixed with lime water I call *incorporated oil.*

The Method of preparing, and the Expense of the various Impenetrable Paints.

First.—*Subdued Green.*

	£.	s.	d.
Fresh lime water, 6 gallons -	0	0	3
Road dirt finely sifted, 112 pounds	0	1	0
Whiting, 112 ditto - -	0	2	4
Blue black, 30 ditto - -	0	2	6
Wet blue, 20 ditto - -	0	10	0
Residue of the oil, 3 gallons -	0	6	0
Yellow ochre in powder, 24 pounds	0	2	0
	1	4	1

This composition will weigh 368 pounds, which is scarce one penny per pound. To render the above paint fit for use, to every eight pounds add one quart of the incorporated oil and one quart of linseed oil, and it will be found a paint with every requisite quality, both of beauty, durability, and cheapness; and in this state of preparation does not exceed twopence-halfpenny per pound, whereas the coal tar of the same colour is sixpence.

The Method of Mixing the Ingredients for the subdued Green.

First, pour six gallons of lime water into a large tub, then throw in 112 pounds of whiting; stir it round well with a stirrer, let it settle for about an hour, and stir it again. Now you may put in the 112 pounds of road dirt, mix it well, then add the blue black, after which the yellow ochre, and, when all is tolerably blended, take it out of the tub and put it on a large board or platform, and with a labourer's shovel mix and work it about as they do mortar. Now add the wet blue, which must be previously ground in the incorporated oil (as it will not grind or mix with any other oil). When this is added to the mass, you may begin to thin it with the incorporated oil in the proportion of one quart to every eight pounds, and then the linseed oil in the same proportion, and it is ready to be put into casks for use.

Lead Colour.

	£.	s.	d.
Whiting, 112 pounds - -	0	2	4
Blue black, 5 ditto - -	0	1	8
Lead ground in oil, 28 ditto -	0	14	0
Road dirt, 56 ditto - -	0	0	6
Lime water, 5 gallons - -	0	0	6
Residue of the oil, 2½ ditto -	0	5	0
	1	4	0

Weighs 256 pounds.

To the above add two gallons of the incorporated oil, and two gallons of linseed oil to thin it for use, and it will not exceed 1¾*d.* per pound.

Note. The lime water, whiting, road dirt, and blue black, must be first mixed together; then add the ground lead, first blending it with two gallons and a half of the prepared fish oil; after which thin the whole with the two gallons of linseed oil and two gallons of incorporated oil, and it will be fit for use. For garden doors, and other work liable to be

in constant use, a little spirits of turpentine may be added to the paint whilst laying on, which will have the desired effect.

Bright Green.

	£.	s.	d.
112 pounds yellow ochre in powder, at 2*d*. per lb.	0	18	8
168 ditto road dust - - -	0	1	8
112 ditto wet blue, at 6*d*. per lb. -	2	16	0
10 ditto blue black, at 3*d*. ditto -	0	2	6
6 gallons of lime water - -	0	0	6
4 ditto fish oil prepared - -	0	12	0
7½ ditto incorporated oil - -	0	15	0
7½ ditto linseed oil, at 4*s*. 6*d*. per gallon	2	8	9
592 lbs. weight	7	15	1

This excellent bright green does not exceed threepence farthing per pound ready to lay on, and the inventor challenges any colourman or painter to produce a green equal to it for eighteen-pence.

After painting, the colour left in the pot may be covered with water to prevent it from skinning, and the brushes, as usual, should be cleaned with the painting-knife, and kept under water.

A brighter green may be formed by omitting the blue black; and

A lighter green may be made by the addition of ten pounds of ground white lead.

A variety of greens may be obtained by varying the proportions of the blue and yellow.

Observe that the wet blue must be ground with the incorporated oil preparatory to its being mixed with the mass.

Stone Colour.

	£.	s.	d.
Lime water, 4 gallons - - -	0	0	4
Whiting, 112 pounds - - -	0	2	4
White lead ground, 28 pounds, at 6*d*. per lb.	0	14	0
	0	16	8

Road

		£.	s.	d.
	Brought over	0	16	8
Road dust, 56 pounds	- - -	0	0	6
Prepared fish oil, 2 gallons	- -	0	6	0
Incorporated oil, 3½ gallons	- -	0	7	0
Linseed oil, 3½ ditto	- - -	0	15	9
Weighs 293 lbs.		2	5	11

The above stone colour, fit for use, is not twopence per pound,

Brown Red.

		£.	s.	d.
Lime water, 8 gallons	- -	0	0	8
Spanish brown, 112 lbs.	- -	1	0	0
Road dust, 224 lbs.	- -	0	2	0
4 gallons of fish oil	- -	0	12	0
4 ditto incorporated oil	- -	0	8	0
4 ditto linseed oil	- - -	0	18	0
Weighs 501 lbs.		2	0	8

This most excellent paint is scarcely one penny per pound.

The Spanish brown must be in powder.

A good chocolate colour is made by the addition of blue black in powder, or lamp black, till the colour is to your mind, and a lighter ground may be formed by adding ground white lead.

Note. By ground lead is meant white lead ground in oil.

Yellow is prepared with yellow ochre in powder, in the same proportion as the Spanish brown.

Black is also prepared in the same proportion, using lamp black or blue black.

To whiten Linseed Oil,

Take any quantity of linseed oil, and to every gallon add two ounces of litharge; shake it up every day for fourteen days, then let it settle a day or two; pour off the clear into shallow pans, the same as dripping-pans, first putting half a pint of spirits of turpentine to each gallon. Place it in the sun, and in three days it will be as white as nut oil.

This oil, before it is bleached, and without the turpentine, is far superior to the best boiled oil, there being no waste or offensive smell *.

P. S. I beg leave here to subjoin a receipt for a constant white for the inside painting of houses, which paint, though not divested of smell in the operation, will become dry in four hours, and all smell gone in that time.

White Paint.

To one gallon of spirits of turpentine add two pounds of frankincense; let it simmer over a clear fire until dissolved; strain it, and bottle it for use. To one gallon of my bleached linseed oil, add one quart of the above; shake them well together, and bottle it also. Let any quantity of white lead be ground with spirits of turpentine very fine, then add a sufficient portion of the last mixture to it, until you find it fit for laying on. If in working it grows thick, it must be thinned with spirits of turpentine. It is a flat or dead white.

No. 21, Mary-le-bone-street; Golden-square,
April 9th, 1805.

The above communication was accompanied with the following certificate:

Letter to Mr. VANHERMAN *from Mr.* W. HILL, *Builder and Surveyor to his Grace the Duke of Richmond.*

SIR,

I have just received your letter dated the 5th instant, and am happy to find that your oil and colour business so well stands the test of others as well as that of myself. The fish oil composition you made use of in all the painting you have done at Earl's Court, Kensington, for his grace the duke of Richmond, under my superintendance, in 1802-3, was fully equal if not superior to any painting done in the usual way with linseed oil, white lead, &c. I have also the

* From experiments made, it appears that fine sand will not answer the purposes of road dirt in painting, and that this dry dirt or dust collected in highways much travelled by horses and carriages, and afterwards finely sifted, is the article recommended as possessing the properties required.

highest

highest opinion of your coarse composition and fish oil you made use of on the out-buildings, fences, &c. on the above premises; the great body and hard surface it holds out, must be of the greatest preservation to all timbers and fences exposed to open air and all weathers. It must also be of the greatest service on plastered stucco, external walls, &c.

If any further attestation from me relative to the business you did at the above premises can be of any service to you, you will command,

Sir, your obedient servant,

West Lavant, near Chichester, Sussex,
February 7, 1805. W. Hill.

IX. *Chemico-Galvanic Observations, communicated to the National Institute of Italy. By M. L.* Brugnatelli*.

§ I. Pavia, Sept. 23, 1805.

Muriatic Acid may be obtained by Galvanizing Water with Gold, Platina, Iron, and Oxide of Manganese.

It is a long time since several chemists, who had turned their attention to Galvanic experiments, discovered that muriatic acid might be produced by these processes. Mr. Simon, of Berlin, the first who made this curious observation, published it in the Journal of Gilbert in 1801. He made use of two tubes filled with distilled water. They were closed at the bottom, and placed in communication by means of a muscular fibre, and a gold wire was passed into each tube through the corks; one of these wires communicated with the positive pole of a pile of Volta, and the other with the negative pole. At the end of twenty-four hours the water on the zinc side of the pile assumed a yellow tint, and it liberated a good deal of oxygen gas. This yellow water had the smell of the oxymuriatic acid; it whitened the cork; it reddened the tincture of turnsole; it occasioned an effervescence with the carbonate of potash, with which it afterwards yielded cubical crystals which decrepitated in the

* An extract from *Osservazioni Chimico-Galvaniche, &c.*

fire,

fire, and the solution of which decomposed the nitrate of silver and converted it into muriate of silver. The above chemist, therefore, had obtained, by means of Galvanism, muriatic acid at first, and afterwards the same acid oxygenated, which dissolved gold.

He tried to vary the apparatus by employing only one tube bent in the form of the letter V. A gold wire was inserted into each branch, and communicated with the two opposite poles of the pile. He did not then obtain any appearance of muriatic acid in the branch on the side of the positive pole; and we shall soon see that this acid could not be formed there, because the water of this branch was in free communication with that upon which the negative pole acted. This difference in the results made him suppose that the formation of the muriatic acid in his first experiments depended upon the presence of the muscular fibre which formed part of the Voltaic circuit.

Cruickshank (Additional Remarks on Galvanic Electricity, 1801), on plunging in a solution of muriate of lime a gold wire communicating with the positive pole of a pile, discovered that, after having completed the Voltaic circuit, the water of the solution was decomposed, that the liquor became yellow, assumed the smell of aqua-regia, and that the gold wire was attacked. Brugnatelli, upon repeating this experiment, always observed a yellowish precipitate, which he discovered was not lime, the base of the muriate, but a true oxide of gold.

By proceeding in the same manner with a solution of muriate of soda, Cruickshank also obtained oxymuriatic acid, but without the decomposition of any part of the water; so that this acid must have been of new formation. Brugnatelli, upon repeating the experiment with gold wires in solutions of muriate of potash and ammonia, likewise saw oxymuriatic acid formed by the Galvanic action.

He observes, that the muriates which dissolve in very little water, as muriate of lime, are the most prompt in evincing the production of oxymuriatic acid by means of Galvanism.

The simple muriatic acid which is produced in water positively

sitively galvanized, as well as that which we have mentioned, is converted afterwards into the oxygenated acid by the oxygen generated during the process. This is an experiment often repeated by the author, and always with success.

Pacchiani was the first in Italy who obtained muriatic acid upon galvanizing with gold wires pure distilled water, after the manner of Simon: but this water, being always in contact with animal or vegetable matter in the apparatus, it was supposed that these substances had some share in this production. Several chemists also believed that the muriatic acid procured by Cruickshank and Brugnatelli from the solution of muriates was not a new formation, but proceeded from a partial decomposition of these saline substances. In order to settle the dispute, the author galvanized with very fine gold wires distilled water, which had no connection whatever with any vegetable or animal substances, or any muriate. His apparatus was a simple glass tube closed at bottom and filled with distilled water, in which was inserted a very fine wire of *pure* gold (a very essential requisite), or platina, placed in contact with the positive pole of a pile. The circuit was completed by another tube, bent, four lines in diameter, equally full of distilled water, and which passed from the tube in which the gold wire was inserted into a dish full of water, which was placed in communication with the negative pole by means of a strip of tin, or of caoutchouc softened in warm water.

In the first experiment made with this apparatus, the water galvanized for some hours reddened the blue tinctures strongly; but the most sensible metallic re-agents did not affect them, and consequently did not indicate, by any certain sign, the presence of the muriatic acid.

The author tried to carry up his pile to a hundred pair of disks of two inches, whereas it was only fifty before; he submitted to this new apparatus distilled water contained in tubes a little larger than a large writing-quill, and two inches long, by employing wires of very pure gold, which he prepared expressly for the purpose; he then ascertained that the acid obtained without the intervention of any organical substance

substance was the true muriatic acid. It had the same smell; it quickly whitened the nitrates of silver and of mercury, and turned the blue tinctures into a lively red.

With the same apparatus the author afterwards obtained muriate of soda upon galvanizing with a gold wire a weak solution of pure soda until all the alkaline character had disappeared.

Upon galvanizing with an iron wire instead of a gold one pure water with the positive pole of the pile for sixteen hours, he obtained a white precipitate, which became a bright blue with the prussiate of potash, and black with the infusion of galls.

In lime water, submitted to the same process, the decomposition of the water was very rapid. The liquid, when reduced to two-thirds of its volume, exhaled the smell of the muriatic acid and reddened the blue tincture a little; it was formed of muriate of lime, precipitable by potash but not at all by oxalic acid, which is consequently a re-agent, not to be trusted for detecting lime in its various combinations, particularly when they are acidulated.

Induced by theoretical reflections, the author tried to substitute in place of gold the crystallized oxide of manganese in the Galvanic decomposition of water. He filled two tubes, in each of which was plunged a piece of this oxide suspended by a copper wire: one of these wires touched the negative and the other the positive pole of the pile; the two tubes were placed against the inner side of a conical drinking-glass, at the bottom of which water completed the circuit. The water in the tubes was in contact with the manganese only, and not touching the copper. At the end of 24 hours the water of the positive tube contained muriatic acid united to a little manganese, by means of which the silver precipitated from its solution assumed an obscure red tint. The water of the positive tube was strongly alkaline. There had been a considerable disengagement of gas in the other tube. The manganese, however, on the negative side did not appear to have been reduced by the influence of the hydrogen in the act of its formation.

§ II.

Muriatic Acid is not obtained with all the Metals from pure Water positively galvanized.

The author, proceeding in a course of experiments upon the reciprocal effects of water and of various metals when submitted together to the Galvanic action, began with silver. He placed two thick wires of this metal, as fine as he could procure it, in two separate tubes filled with distilled water, closed at bottom with parchment, and supported together against the side of a drinking-glass, at the bottom of which water completed the circuit. The wires approached two opposite poles of a pile of two columns, which communicated together by means of a stripe of metal. After being galvanized some hours, the surprise of the author was very great upon finding the water of the positive tube not at all acidulated, as it was when gold or platina wires were employed, but very decidedly alkaline. The experiment was often repeated with silver wires still finer, and always with the same result.

Upon plunging, during a whole night, two silver wires coming from the two poles of the pile in the same vessel full of water, at the distance of some lines from each other, alkalized water was likewise obtained.

Copper wires in separate tubes filled with water, yielded by the Galvanic action (continued for 12 hours) a good deal of hydrogen gas on the side of the negative pole. On the other side, no gas nor any sign of oxidation was obtained. The water in the two tubes was alkalized. Antimony produced the same effects.

Among the metals employed by the author as agents in these Galvanic decompositions, tin and zinc evinced themselves the most energetic in rendering the water alkaline on the side of the negative pole, where, at the same time, a good deal of hydrogen gas was liberated. It was only after a very long time that signs of alkalinity were observed on the side of the positive pole.

Two straight stripes of tin, which communicated by one of each of their extremities with the two poles of the pile, while

while their other extremities were inserted in the water of the two separate tubes, produced the following effects:—there was, as usual, a good deal of gas disengaged at first from the negative side, without any sensible alteration in the metal, although it was slightly blackened. On the positive side no bubbles were perceived, but a good deal of white oxide was formed, which made the water milky, and was partly precipitated. At the end of six hours of Galvanic action the water was neither acidulated nor alkaline; but in twelve hours it changed the blue tincture of althæa into a green.

When the two stripes of tin were inserted in one vessel, the water constantly became alkaline and of a milky colour from the effects of the precipitate on the positive side.

Upon substituting zinc in place of tin in two separate tubes, signs of alkalinity were very speedily obtained in the water on the negative side; but on the other side very weak signs only at the end of twenty-four hours. Both of the stripes of zinc were equally covered with a blackish coat, which the author supposed to be the hydrogenated metal.

§ III.

Experiments which demonstrate that, in the Decomposition of Water produced with Gold Wires attached to the positive Side of the Pile, Muriatic Acid is not always formed.

The author galvanized nitric acid for twelve hours with a gold wire attached to the positive pole of the pile. Oxygen gas was continually liberated. The acid did not undergo any perceptible change, and the gold was not dissolved, which proves that aqua regia was not formed in the process. Cruickshank, when galvanizing positively this same acid with platina wires, and Davy with gold wires, did not produce any change upon it: but Vassalli-Eandi has maintained that if this same acid is very much concentrated it is decomposed by Galvanism.

Upon treating in the same manner on the positive side of the pile a solution of crystallized acetate of lead, the gold wire became speedily black towards the highest part introduced; a little lower, a deep red; and at last, citron. After twenty-six hours continuance in the Galvanic circuit with-

out

out the appearance of gas, the surface of the gold was covered with a substance of a brilliant black. When the gold wire was taken away the solution remained transparent, without the least appearance of muriate of lead, which would have been manifested if there had been any production of muriatic acid. The oxygen, then, was liberated from the water, and the black coating which covered the gold in the preceding experiment was hyper-oxygenated lead, as Ritter had observed. It was separated in small brittle fragments like glass. It was an exciter and a conductor of Galvanism; it converted into muriatic acid the oxymuriatic acid gas, by changing itself, in sight of the operator, into very white muriate of lead. Finally, when mixed with phosphorus, it fulminated by means of percussion.

When it was tried to substitute the solution of nitrate of silver in place of that of acetate of lead, no precipitate was obtained at the end of fifteen hours of Galvanic action; but there was hyper-oxygenated oxide of silver.

On Galvanizing for two days black oxide of mercury in a straight tube full of distilled water, and communicating with the positive side of the pile by a gold wire, the author, in a great measure, converted the black into the red oxide; the water of the tube reddened a little the blue tinctures, but it did not contain an atom of muriate of mercury.

§ IV.

Oxidulated Oxide of Mercury, as well as sweet Mercury, may be obtained by the Galvanic Action.

The author galvanized on the positive side, in a separate tube, and with a gold wire, a dilute solution of nitrate of mercury made in the cold. At the end of a few hours the wire was covered with very small yellowish crystals of a prismatic form. This salt was not the ordinary muriate of mercury, which is opake, very white, and does not crystallize. It was not a nitrate of mercury, because this is soluble in water. It became as black as charcoal when put into lime water; and the author is persuaded, from what he remarked, that it was true sweet mercury.

§ V.

§ V.

Nitric Acid cannot be obtained in Water galvanized positively with a Gold Wire.

Cruickshank and other chemists have supposed that the acid formed in water exposed to the Galvanic action on the positive side of the pile is nitric acid. The author observes upon this occasion, that silver, which he had often submitted to this influence, never yielded (however long or vigorous the Galvanic action) any thing else than a brownish gray precipitate without the least symptom of nitrate, which, being very soluble, ought to have manifested itself in the water. Cruickshank, in maintaining the existence of this salt in the above case, tries to explain its insolubility by attributing it to an excess of base as well as of oxide of silver.

The author, in order to ascertain if the precipitate of silver thus obtained contained any nitric acid, treated it with pure potash; and he never obtained the slightest appearance of nitre.

He placed this precipitate of silver, recently produced, in a little tube full of pure water, and galvanized it a whole day with a gold wire attached to the positive side of a strong pile. The precipitate remained insoluble; and the water did not appear to contain an atom of any salt with a base of silver. We shall speak by and by of the difference which exists between the deposit furnished by silver wire galvanized in water, according as it answers the positive or negative side of the pile.

§ VI.

Upon the Nature of the Alkali formed in Water by Galvanism.

All the chemists who supposed that they had formed an alkali in water galvanized by the negative side of the pile, believed that this alkali was ammonia. Its formation, from the hydrogen liberated in the process, and a little azote, which may be met with in water even when distilled, appeared natural enough. Our author, however, who never found

found the characteristics of ammonia in water thus alkalized, opposes this opinion. This water speedily changed into green the spirituous tincture of purple althea, a re-agent, according to Brugnatelli, the most sensible of all others to the presence of the alkalis; it whitened the solution of silver: sometimes this alkali is effervescent, and precipitates lime in lime water; but it is so diluted in water, that it gives it no sensible taste.

An experiment, made by the author two years ago, had already proved that Galvanism does not form ammonia. He placed the black oxide of mercury at the bottom of a tube filled with distilled water, into which a gold wire entered in communication with the negative pole of a pile. At the end of twenty-four hours this wire was covered with revived mercury wherever it had touched the water. This water remained insipid, but it quickly turned the infusion of althea into green. It might be pretended that it contained ammoniuret of mercury, but no traces of this metallic combination were found in it; and although it is always formed by a prolonged contact of ammonia (even when very much diluted) with oxide of mercury, yet water alkalized by means of Galvanism, however long continued upon this same oxide, produces no ammoniuret.

In order to determine, in short, which of the known alkalis existed in water negatively galvanized, the author distilled two pounds of this liquid, alkalized by Galvanism in various experiments, with a stripe of tin foil which came in contact with the negative pole of some very energetic piles. When the distillation was well advanced, pure water was found in the receiver, and the residue in the retort had the smell of fixed alkalis. Muriatic acid was added to it until saturation: it was evaporated slowly in the air, and a great many small cubes of muriate of soda were obtained.

The author repeated this interesting experiment several times, and always with the same success. Supposing that the soda might come from the pile, the pasteboard disks of which were impregnated with a solution of common salt, which had been decomposed by the action of the zinc (as it has often done), and made its alkaline base appear out of

the pile under the form of an efflorescence; he repeated the experiment with a pile of a hundred pairs of very good metallic disks, separated by small rounds of pasteboard moistened with pure water. This pile not seeming active enough, he steeped the disks of pasteboard in a solution of sulphate of magnesia (Epsom salt); the decomposition of the water succeeded then very well, and the alkali produced was again soda.

As muriatic acid was formed by galvanizing water by the positive pole, and soda by the negative pole, the author flattered himself that by plunging in the same pure water two gold wires (each of which operated in its own way, *i.e.* the one negatively and the other positively), by means of the action of a strong pile of a hundred pairs, he could obtain common salt; but although, in consequence of a Galvanic action thus established and continued for two days, the water had considerably diminished, not the least change was observed, as had been already remarked by Pacchiani. There was no peculiar smell or taste, no effect upon blue tinctures, no precipitate in metallic solutions; and, upon evaporation, not an atom of any salt whatever.

But after having galvanized several times, both negatively and positively, a certain quantity of pure water with gold wires inserted in separate tubes, until the re-agents gave unequivocal signs of the production of the acid on one part, and of alkali on the other, the two liquids mixed to perfect saturation, and evaporated in the air, always gave cubical crystallized muriate of soda. No doubt, therefore, remains with the author, that water, negatively and positively galvanized by gold wires, produces or disengages muriatic acid in the one case, and soda in the other. But he observes, that the formation of the alkali by the effect of the negative pole of the pile is quicker and more abundant than the formation of the acid by the influence of the opposite.

[To be continued.]

X. *New*

X. *New Process for clearing Feathers from their animal Oil. By Mrs.* Jane Richardson, *Willis's Place, Chelsea**.

In the session of the Society of Arts, &c. 1805, twenty guineas were adjudged to Mrs. Richardson, being the premium offered for the best process for clearing feathers.

The Process.

Take for every gallon of clean water, one pound of quick-lime; mix them well together, and when the undissolved lime is precipitated in fine powder, pour off the clear lime-water for use, at the time it is wanted.

Put the feathers to be cleared in another tub, and add to them a quantity of the clear lime-water, sufficient to cover the feathers about three inches when well immersed and stirred about therein.

The feathers, when thoroughly moistened, will sink down, and should remain in the lime-water three or four days, after which the foul liquor should be separated from the feathers by laying them on a sieve.

The feathers should be afterwards well washed in clean water and dried upon nets; the meshes about the fineness of cabbage-nets.

The feathers must from time to time be shaken upon the nets, and as they dry will fall through the meshes, and are to be collected for use.

The admission of air will be serviceable in the drying. The whole process will be completed in about three weeks; after being prepared as above mentioned, they will only require beating for use.

Mr. Jolly, poulterer, of Charing-cross, attended a committee of the society appointed to inspect the feathers, and stated that Mrs. Richardson had bought from him forty pounds weight of feathers, in the state they were plucked from dead geese, and in such a condition that if they had been kept in the bag only four days, without being cleansed,

* From *Transactions of the Society of Arts,* &c. 1805.

they would have been very offensive; that the feathers exhibited by Mrs. Richardson appear to be the same he had sold her, but that they were now in a much cleaner state, and seem perfectly cleared from their animal oil.

The committee, in order to authenticate more fully the merits of Mrs. Richardson's process, requested Mr. Grant, a considerable dealer in feathers, to furnish some specimens of feathers of different kinds in an unclean state, to be cleansed by Mrs. Richardson; in consequence whereof an application was made to Mr. Grant, and the following letter received from him:

SIR,

I take the liberty of sending herewith three samples of feathers, on which the experiments may be tried; but should the quantity not be sufficient, on being favoured with your commands, shall with pleasure send any quantity necessary.

The bag No. 1, contains the commonest feathers we ever make use of—it is a Russian produce of various wild fowl; No. 2, gray Dantzick goose; No. 3, a superior kind of Dantzick goose.

The two first are in their raw state, just taken out of the bags in which they were imported; the last have been stoved the usual time (three days), but retain their unpleasant smell. Should it not be considered giving you too much trouble, shall be extremely obliged by your favouring me with a line when the experiment has been made, and I shall be happy in waiting upon you to know the result.

I am respectfully, Sir,
Your obedient humble servant,
THOMAS GRANT.

No. 226, Piccadilly.

After the feathers last mentioned were sent back by Mrs. Richardson, Mr. Grant attended to examine them, and declared that they appeared to be perfectly well cleaned.

Certificates from Mr. Christopher Bushnan, No. 10, Beaufort-row, Chelsea, and from Mr. W. Baily, testified to the efficacy of Mrs. Richardson's process.

XI. Twenty-ninth Communication from Dr. THORNTON, relative to Pneumatic Medicine,

To Mr. Tilloch.

No. 1, Hinde-street, Manchester-square, May 20, 1806.

SIR,

THE following is a case worthy of record:

Fits and Blindness, arising from Water in the Brain, cured by the Inhalation of Vital Air.

Charlotte Durand, residing at No. 101, Jermyn-street, her father a shoe-maker, for five months had been afflicted with fits, frequently from eight to ten in a day. The duration of these convulsions was from ten minutes to a quarter of an hour. At length she became perfectly blind; and she had been in this state three weeks when she first came under my care. The plan of treatment was as follows:

Thursday, November 24, 1795.

℞ Rhei pulv. .. .. .. gr. 10
Kali vitriolat. .. .. scr. 1
Syr. simp. .. .. .. dr. 2
Aq. cinnam.
Aq. menth. pip. *āā* .. dr. 10
F. Haustus catharticus.

Direction.

Take a third of this draught going to bed, and the remainder the next morning early.

Saturday, November 26.

℞ Extr. cinchon.
Fer. vitriolat. *āā* .. .. dr. ½
F. Pil. 20

Directions.

1. Let her take one pill at night, and a third of the opening draught before ordered; and the remainder early the next day with another pill.
2. On Sunday night she is to take two pills at bed-time.
3. One pill fasting and two at eleven on Monday.
4. To inhale the *vital air*, one quart, diluted with twenty of atmospheric air.

 Monday,

Monday, November 28.

1. Take one pill early in the morning, at eleven, and at six in the evening each day, dissolving it in two table spoonfuls of the mixture as below:

℞ Aq. cinnam.

Aq. menth. pip. āā	..	unc.	2
Syr. simp.	..	unc.	2
Cinchon. pulv. ..	..	dr.	$1\frac{1}{2}$

F. Mist.

2. Vital air to be continued.

Wednesday, December 7.

As last ordered.

N. B. sight returned half an hour after inhaling the vital air on last Friday.

The fits have entirely disappeared.

Saturday, December 16.

Take the opening draught, as ordered November 24, and come here Monday next.

The appearance of health. No disease.

Monday, December 25.

Dismissed as cured.

Observations on this Case.

1. The marked symptoms in this case, with the duration of the disease, and the sudden return of vision so quickly after inhaling a superoxygenated air, is a strong proof of the efficacy of the means employed.

2. These fits, with the blindness, arose from the gradual deposition of water in the brain.

3. The absorption was occasioned by the powerful action of the vital air on the absorbents, aided by other tonic medicines.

4. Under the other usual practices, this disease commonly terminates in death; and even when cured by oxygenated mercury leaves the constitution so enfeebled, that the patient soon after falls a victim to some other disease.

I remain, dear Sir,

Your's truly,

ROBERT JOHN THORNTON.

XII. *New Method of cleansing Silk, Woollen, and Cotton Goods, without Damage to the Texture or the Colour. By Mrs.* Ann Morris, *of Union Street**.

Fifteen guineas were voted by the Society of Arts to Mrs. Morris, for communicating this new process, which is as follows:

Take raw potatoes, in the state they are taken out of the earth, wash them well, then rub them on a grater over a vessel of clean water to a fine pulp, pass the liquid matter through a coarse sieve into another tub of clear water; let the mixture stand till the fine white particles of the potatoes are precipitated, then pour the mucilaginous liquor from the fecula, and preserve this liquor for use. The article to be cleaned should then be laid upon a linen cloth on a table, and having provided a clean sponge, dip the sponge in the potatoe-liquor, and apply the sponge thus wet upon the article to be cleaned, and rub it well upon it with repeated portions of the potatoe-liquor, till the dirt is perfectly separated; then wash the article in clean water several times, to remove the loose dirt; it may afterwards be smoothed or dried.

Two middle-sized potatoes will be sufficient for a pint of water.

The white fecula which separates in making the mucilaginous liquor will answer the purpose of tapioca, will make an useful nourishing food with soup or milk, or serve to make starch and hair-powder.

The coarse pulp which does not pass the sieve is of great use in cleaning worsted curtains, tapestry, carpets, or other coarse goods.

The mucilaginous liquor of the potatoes will clean all sorts of silk, cotton, or woollen goods, without hurting the texture of the article, or spoiling the colour.

It is also useful in cleansing oil paintings, or furniture that is soiled.

Dirty painted wainscots may be cleaned by wetting a

* From *Transactions of the Society of Arts; &c.* 1805.

sponge in the liquor, then dipping it in a little fine clean sand, and afterwards rubbing the wainscot therewith.

Various experiments were made by Mrs. Morris in the presence of a committee, at the society's house: the whole process was performed before them upon fine and coarse goods of different fabrics, and to their satisfaction.

XIII. *On Electricity.*

To the Editor of the Philosophical Magazine.

SIR,

A few evenings ago, when making some electrical experiments, the following circumstance occurred, which was quite unexpected, and which, I believe, has been very little noticed hitherto. The phænomenon related by your correspondent C. R. in your numbers for March 1805 and last March, appears to be occasioned by the same cause.

Happening to take two cylindrical pieces of wood, covered with tin-foil, in my hands to discharge a loaded jar—on removing these cylinders from each other (after the discharge had been made) there was a small adhesion between them, which I at first thought might be occasioned by a piece of wax, but this was not the case; this adhesion I perceived several times that evening, and also since, and sometimes there was a small protuberance raised at the part where the cylinders touched; I also perceived the same sort of adhesion between one of the cylinders and the coating. The phænomenon alluded to above, of your correspondent's, is that of a shilling, or other piece of metal, which being placed between the knob of the discharger and the coating of a jar adhered to the side of the jar after the discharge: this experiment I tried and succeeded in; a shilling adhered for longer than twelve hours to the coating, and perhaps if it had not been taken off might have kept on for days. Whether this adhesion and that before mentioned betwixt the cylinders were caused by a fusion of the tin-foil or not, I will not take on me to determine. I once conjectured that this effect was

was caused by a vacuum (of air) being made by the passage of the electric fluid: but I scarcely think, had this been the case, that the shilling would have adhered for so many hours as it did. I hope your readers will be induced to make experiments for the purpose of determining the cause of the adhesion, which appears to be a remarkable circumstance, and worth attending to.

I am, &c.

May 7, 1806. *An occasional Correspondent.*

XIV. *Letter from* Ez. Walker, *on his Instrument called a Cometarium, described in our last Volume.*

To Mr. Tilloch.

Sir, Lynn, May 31, 1806.

The editor of the Retrospect of Philosophical, Mechanical, Chemical, and Agricultural Discoveries, after having copied from your Magazine, No. 93, most part of my paper respecting an instrument which I have called a *cometarium*, makes the following observations:

"This contrivance is founded upon a property of the convex lens, which does not appear to be generally known: it will probably furnish a representation of the figure of a comet more accurate and natural in most respects than has hitherto been obtained. But the name of *cometarium* is not very appropriately applied to the instrument, since it neither exhibits the relative magnitudes of the sun and comet, nor nearly the true orbit and motion of the latter. Indeed, there is reason to apprehend that the contrivance, so far from serving as a cometarium in its present state, cannot even be adapted successfully to any system of machinery which will correctly exhibit the phænomena of the solar system." —*Retrospect*, vol. ii. p. 20.

The objections made by this gentleman to the name of my instrument do not appear to be well founded; for a planetarium neither exhibits the relative magnitudes, distances, motions, nor the true figures of the orbits of the planets, and yet

yet the instrument is called a planetarium notwithstanding. Nor, indeed, has any system of machinery yet been made "which will correctly exhibit the phænomena of the solar system."

"An orrery is a very fit machine to show the system of the world, and some of them have been made at an enormous expense, with a great multiplication of wheels and other parts, by which means they have imitated the principal movements of the celestial bodies; but even the best of them fall very short of real accuracy, and of course they are quite unfit for the purposes of calculating the future situations of the celestial bodies."—*Cavallo's Elements of Philosophy*, vol. iv. p. 281.

My cometarium was never intended to be adapted to any system of machinery made for exhibiting the phænomena of the solar system; for those phænomena are so various, that it is found expedient to exhibit different parts of the system by different machines; and, perhaps, this is the best way of exhibiting the phænomena of a comet. The instrument described in my former paper was intended only to represent a comet revolving in a circular orbit, with its tail turned from the sun; but I have since added machinery, constructed on the principle of the elliptical compasses, by which a comet may be represented as describing an elliptical curve, either accompanied with a tail as before, or when deprived of that luminous appendage, by taking away the great lens.

The sun is now represented by a lamp placed in a lantern with lenses so adjusted as to throw a circular spot of light upon the screen. This lantern stands between the cometarium and the screen, but unconnected with either, so that the sun is represented as stationary in the focus of the ellipse which the comet describes. Although the instrument is turned only by hand, yet by this means the relative motion of a comet may be shown sufficiently near the truth for conveying a general idea; but those who wish for a more regular motion may easily add machinery for that purpose. The effects of this apparatus are seen to advantage upon a transparent screen, and, when used in this manner, it promises to be useful in a lecture-room.

The

The optical principle on which this instrument is constructed seems to merit more attention than I have hitherto been able to pay to it; and in the instrument itself there is still room for further improvements.

I am, Sir,

Your humble Servant,

Ez. Walker.

XV. *Notices respecting New Books.*

Philosophical Transactions of the Royal Society of London, for the Year 1806. *Part I.*

This part contains:—1. The Croonian Lecture on the Arrangement and mechanical Action of the Muscles of Fishes. By Anthony Carlisle, Esq. F.R.S. F.L.S.—2. The Bakerian Lecture on the Force of Percussion. By William Hyde Wollaston, M.D. Sec. R.S.—3. Mémoire sur les Quantités imaginaires. Par M. Buée.—4. Chemical Experiments on Guaiacum. By Mr. William Brande.—5. On the Direction of the Radicle and Germen during the Vegetation of Seeds. By Thomas Andrew Knight, Esq. F.R.S. In a Letter to the Right Hon. Sir Joseph Banks, K.B. P.R.S.—6. A third Series of Experiments on an artificial Substance which possesses the principal characteristic Properties of Tannin; with some Remarks on Coal. By Charles Hatchett, Esq. F.R.S.—7. The Application of a Method of Differences to the Species of Series whose Sums are obtained by Mr. Landen by the Help of impossible Quantities. By Mr. Benjamin Gompertz.—8. An Account of a small Lobe of the human prostate Gland, which has not before been taken notice of by Anatomists. By Everard Home, Esq. F.R.S.—9. On the Quantity and Velocity of the Solar Motion. By William Herschel, LL.D. F.R.S.

Appendix.—Meteorological Journal kept at the Apartments of the Royal Society, by Order of the President and Council.

An

An Epitome of Chemistry. By WILLIAM HENRY. *The Fourth Edition,* 8*vo. Published by Johnson. Price* 12*s.*

With great pleasure we announce this useful publication, which may be considered as a new work, though modestly announced as only a new edition of the work published before under the same title. We consider this as one of the very best works that has yet appeared on the subject of which it treats. While it presents a well arranged collection of chemical facts and processes, it is at the same time well adapted to direct a learner to such a train of simple and easy experiments as may best enable him to make rapid progress in acquiring correct ideas of the theory. It embraces all the new facts that have arisen since the last edition was published, and is illustrated with engravings of every article of apparatus that is essential to the pursuit of operative chemistry. They are by Lowry, and executed with his usual accuracy, precision, and elegance.

An appendix to the work contains a number of useful tables, several of which are entirely new.

A Chemical Catechism. By S. PARKES, *Manufacturing Chemist.* 8*vo. Published by Symonds. Price* 12*s.* 624 *Pages.*

The object of this publication is to unfold the science of chemistry to artizans and young people by way of question and answer. It is written in an elegant and popular manner, and rendered as amusing as it is instructive. The answers are concise, and fitted for the memory. Occasionally the subject is dilated, and difficulties cleared up by a more ample elucidation, and this is very properly brought forward in the form of notes.

The work is divided into thirteen chapters; viz. Chap. I. Introductory and Miscellaneous: II. Of Atmospheric Air: III. Of Caloric: IV. Of Water: V. Of Earths: VI. Of Alkalies: VII. Of Acids: VIII. Of Salts: IX. Of Simple Combustibles: X. Of Metals: XI. Of Oxides: XII. Of Combustion: XIII. Of Attraction, Repulsion, and Chemical Affinity. Each of these chapters is a *distinct* tract, in

in question and answer, with full explanatory notes neatly printed at the bottom of each page. The Appendix contains 34 pages of *additional* notes, which are extremely interesting; 14 pages of Chemical Tables, some of which are entirely *new*; 154 instructive and amusing Experiments; a very copious Vocabulary of Chemical Terms; and a general Index to the whole.

A few short extracts will be sufficient to point out the method of the author, and the perspicuity with which the several subjects are treated. From Chap. III. Of Caloric, p. 113. "*Will the thermometer show the quantity of caloric in all bodies?* No: it will not show that portion which is latent or *chemically* combined with any body: for instance, fluids require a certain portion of caloric to keep them in a state of fluidity; which portion is *not* indicated by the thermometer. *Is the thermometer, then, of no use in ascertaining the temperature af fluids?* Yes: fluids operate upon the thermometer in the same manner as solids; for whatever *free* caloric be contained in any liquid, *that* portion is accurately shown by the thermometer. *What do you call that portion of caloric which is a necessary part of fluids?* It is called the caloric of fluidity; but different fluids require different portions of it to preserve them in the state of fluids. *What are the effects of caloric upon bodies?* The *general* effects of caloric are to increase the bulk of the substances with which it unites, and to render them specifically lighter than they were before. *What are the particular effects of caloric on bodies?* It favours the solution of salts, and promotes the union of many substances. In other cases it serves to separate bodies already united; so that in the hands of chemists it is the most useful and powerful agent we are acquainted with. *Can you recollect any other effect that caloric has upon bodies?* It is the cause of fluidity in all substances which are capable of becoming fluid, from the heaviest metal to the lightest gas. *How does caloric act upon hard bodies to convert them into fluid?* It insinuates itself among their particles, and separates them from each other. Thus ice is converted into water, and, by a further portion of caloric, into steam."

In the chapter of Earths, each earth is first separately described, with its distinguishing characteristics; then the origin of each; and, lastly, its uses in the arts and manufactures. In the subsequent chapters the *alkalies*, *acids*, *oxides*, and *salts*, are treated in an equally perspicuous manner. The chapter on Metals occupies 72 pages, and is thus divided. First we have the characteristics of metals in general, how they are procured and purified, and how classed by modern chemists. *Each* metal is then separately treated of under five *distinct* heads, thus:—"How is platina *procured?* What is the *nature* of platina? What is the effect of *oxygen* on platina? What *salts* are formed of platina? What are the *uses* of platina?" Direct answers are given to each of these questions, which are further elucidated by a large body of very valuable and learned notes. The chapter concludes with an interesting recapitulation of the general properties of that class of bodies.

A few extracts from the Notes to this excellent work, we trust, will not be unacceptable to our readers. Thus in the chapter on Air: "Every chemist must be aware that a large quantity of *carburetted hydrogen gas* is evolved at the surface of the earth; he must also know that this gas is fatal to animal life. I could adduce a melancholy instance of a gentleman who inhaled it by mistake, and died almost immediately in consequence of it. How then has the all-wise Artificer of the world contrived to protect its inhabitants from the baneful influence of that immense quantity with which the atmosphere is perpetually contaminated? The means are as simple as they are important. Vegetables are so constituted that *carbon* and *hydrogen* are the necessary food of plants, and conduce to the support of vegetable life: their vegetating organs seize the carbonic acid gas which comes within their reach, and, while they appropriate the *carbon* to themselves, the *oxygen* is thrown off to renovate the atmosphere by its union with the nitrogen rejected by animal respiration. Thus, what is noxious to man is rendered beneficial to vegetables; and the oxygen, which vegetables are not in want of, is separated by them in its utmost purity for the use of man." In the chapter on Water the following

following remark occurs:—"Nature, in œconomizing the primary materials of the universe, has constituted *oxygen* the basis both of the atmosphere which surrounds the earth, and of the water which forms its seas and oceans. We see in this and other instances, by what simple means the most beneficial effects have been produced."

> "For mark how oxygen with azote gas
> Plays round the globe in one aërial mass;
> Or, fused with hydrogen, in ceaseless flow,
> Forms the wide waves which foam and roll below."
>
> DARWIN.

We copy the following remark from the chapter on Salts: —"It is remarkable that, though *phosphate of lime* is always found in the urine of adults, this salt is not evacuated by infants. The rapid formation of the bones in the first periods of life requires that there should be no waste of any of the phosphoric salts; and Nature, ever provident, has provided accordingly."

The following note occurs in the chapter on Metals:— "A combination of the white oxide of tin with sulphur, by means of mercury, forms aurum musivum, an article used by artists to give a beautiful colour to bronze. I suspect that the change produced in tin by this process gave rise to the idea of the transmutation of metals. If the alchemists were acquainted with this substance, no wonder that they should indulge the hope of being able to form gold. An experimentalist without theory is the dupe of every illusion." The chapter of Metals concludes with this remark:—"It is no unusual thing for the votaries of chemistry to call it a fascinating science. That it is the most useful of all sciences, cannot be denied; nor can there be the least doubt that it has a strong tendency to enchant those who devote their attention to it. It serves as a powerful stimulus to youth, by occupying their time so satisfactorily to themselves, and rendering all low and unworthy pursuits truly detestable." The chapter of Oxides is closed with the following note:—"All organized beings, whether vegetable or animal, possess the materials of which they are composed

only

only for a limited time: life itself is a boon which is only *lent* to serve the purposes of infinite beneficence. At the proper period *oxygen*, or some other powerful agent, effects the decomposition of the curious fabric, and sets all the elementary particles at liberty, to form other equally perfect and complicated existences,

> "Which thus, alternating with death, fulfil
> The silent mandates of the Almighty's will;
> Whose hand, unseen, the works of nature dooms
> By laws unknown,—who gives and who resumes."

The chapter of Experiments concludes with the following note:—"To read or practise the foregoing experiments merely for the sake of amusement, may occasionally have its advantages; but a resolution to repeat them, and examine all the phænomena, for the sole purpose of receiving instruction, is what the author would principally inculcate. Let it never be forgotten, that *no effect*, however extraordinary, or even trivial, it may appear to us, can ever happen but in consequence of some previously established law of unerring nature. The following apostrophe of Dr. Darwin to the Fountain of all goodness, may possibly tend to impress this important truth upon the student's mind:

> "Thus, at thy potent nod, *effect* and *cause*
> Walk hand in hand, accordant to thy laws;
> Rise at volition's call, in groups combin'd,
> Amuse, delight, instruct, and serve mankind."

Prefixed to this work is "An address to parents on the importance of an early cultivation of the understanding, and on the advantages of giving youth a taste for chemical inquiries," in which the use of chemical knowledge to the superintendants of a variety of manufactories is pointed out in considerable detail, and placed in several striking points of view.

We cannot conclude without strongly recommending this work, not only for the instruction of youth and mechanics, but also to Chemists, as furnishing an admirable epitome of chemistry, and as giving a number of new observations chiefly relating to the manual part of the science. It is not written

written by one in the closet, but by a *practical* chemist by profession. The object is good; and we doubt not its success will correspond with its intrinsic merits.

Mr. Parkinson's second volume of Organic Remains of a former World is in considerable forwardness. Mr. Parkinson has solicited the favour of such remarks and specimens as may aid him in his inquiries respecting corals, the encrinus, star-stones, trochites, entrochites.

M. Vogel, professor of painting in the Lyceum of Warsaw, in Poland, has announced a magnificent work for publication under the title of "Travels of a Painter through Poland:"—It is to be filled with engravings of the scenery of Poland, executed by M. Frey, from professor Vogel's drawings; and will also hand down to posterity engravings of the monuments, palaces, and antiquities, &c. of that country, hitherto so little known. This work of Messrs. Vogel and Frey is the first attempt of the kind in Poland, and deserves the attention of all the friends of science throughout Europe. Three parts are to be published yearly until completed; and the annual subscription is thirty-six rix-dollars. The first part is already published, and is a favourable specimen of the progress of the arts in Poland.

XVI. *Proceedings of Learned Societies.*

ROYAL SOCIETY OF LONDON.

June 5. The President in the chair.—The introductory parts of two mathematical papers were read; the one on the binominal theorem, and the other on a method of facilitating the use of logarithms, and of rendering them more subservient to common purposes.

June 12. The President in the chair.—A curious paper on the variation of the magnetic needle at Jamaica, and its importance in surveying that island, was laid before the society on this evening, with four most magnificent four-sheet

 maps,

maps, by Mr. Robertson. These maps form a complete and superb atlas of Jamaica, and delineate with great elegance its features as they relate to the civil, military, and natural history of that island.

June 19. The President in the chair.—E. Home, esq. furnished an interesting paper on the comparative anatomy and physiology of the camel, particularly on its stomachs and water-bags, or reservoirs, in which it can retain a quantity of water sufficient to support it for several weeks. The camel, like all other ruminating animals, has several stomachs: the *first* receives its food, in which it remains unchanged; the *second*, its water, which also remains pure; the *third* communicates to the water a yellow colour; and the *fourth* unites the contents of the others, and is the general digesting organ of this animal's food. The author confirms the account of this animal given by Buffon, whose vivid eloquence is no where more happily applied, nor more agreeably, than in the history of the camel. Mr. Brandé analysed the water contained in the stomach, and also the animal's urine; in the latter he found, besides carbonate of ammonia, uric acid, which is considered rather singular in this class of animals, and might perhaps be the product of disease, as the creature was old and decaying before it was put to death for the purpose of dissection.

SOCIETY OF ANTIQUARIES.

June 5. Sir H. C. Englefield, bart. vice-president, in the chair.—An account was read of the splendid equipage and sumptuary retinue of the earl of Northumberland at his embarkation for France in the reign of Henry VIII. The Rev. Mr. Milner exhibited a small M.S. volume, bound in red leather, (supposed to be of the time of queen Elizabeth) written on parchment, in Roman characters mixed with Saxon, of the gospel of St. John in Latin. The learned author traced the existence and history of this volume to the time of St. Cuthbert, of Durham, who died early in the sixth century, and contended that it must be more than 1200 years old! To prove this high antiquity, a tradition respecting the funeral of St. Cuthbert was circumstantially detailed,

detailed, and the secret relative to the place of his interment, and the existence (as supposed) of the present M.S. being entrusted to three friars, at the death of one of whom it is to be communicated to another, and in this manner perpetuated to the latest posterity, always known only to *three* friars! The Roman characters of this M.S. are all in capital letters, without any points or marks, and written in short and long lines irregularly.

June 12. The President in the chair.—An etching by Mr. Moore, of the florid gothic church of St. Mary Magdalen, at Taunton, was exhibited; likewise a gun of a construction in use very early after the invention of gunpowder. The stock, which is heavy, is covered with figures partly gilded and delineated in the same style as the paintings in the illuminated M.S.S. of the fourteenth century, and also consisting of scripture history pieces.

Several particulars respecting the death and burial-place (St. John's, Westminster) of sir Walter Raleigh were read, as was a curious account of the embalment, or rather of the preparation of the body of queen Catharine Parr with wax, &c. What will render these facts highly important to many is the following singular circumstance, not generally known; that the tomb of this queen was opened not long since, and the body, more especially the face, found as perfect and as beautiful as the day she died! The action of the atmosphere, however, produced a sensible and instantaneous effect.

June 19. Mr. Orde in the chair.—The Rev. Mr. Stone communicated several decrees or regulations of Henry VIII. and others, which were exhibited, written on parchment in French, and in a black letter character. Their antiquity, and not their contents, constituted their merit.

LONDON CHEMICAL SOCIETY.

It has been observed, that those who cultivate any particular branch of experimental science are solicitous of associating with others engaged in similar studies; a common interest in the same subject of conversation excites a spirit of inquiry; thought gives rise to thought; and new ideas,

collected in the friendly intercourse of society, often lead to investigations of the greatest importance. The student finds many difficulties removed which impede his progress, by the ready information he obtains from men of higher acquirements, whilst those who are skilled in chemical pursuits frequently receive important observations from the mere lover of the science; to this may be added, that men, however great their learning or ardour may be for any particular branch of inquiry, yet, when deprived of the opportunity of communicating their ideas to others, not only become negligent and uninterested in improving the stock of knowledge they already possess, but are seldom solicitous about the further cultivation of their mental powers.

From a conviction of these truths, a number of gentlemen who have a taste for philosophical chemistry are determined to form themselves into a society, in which the talents of a number may be united, and become extensively useful to each other, by mutual communication of their views, their labours, and acquisitions. That their endeavour may prove as interesting as possible, particularly to those promoters of chemical science who cannot devote much time to the perusal of literary journals and periodical publications, arrangements will be made to collect, as speedily as possible, all the chemical news which issue from the laboratories of other operators, both at home and on the continent; and correspondences will be established to obtain the earliest and best information respecting whatever shall offer itself as new and important in the departments of chemistry, of natural philosophy, and the arts and manufactures, which are dependent on these branches of knowledge. To keep pace with the existing state of chemical science, the intelligence thus collected shall be regularly detailed in their respective meetings; and a book of reference kept as a register, containing the growing mass of philosophical information, which will be laid on the table for the use of the members; together with all those publications and academical journals of repute which exhibit the transactions of ingenious men in every part of the world.

The views of this society, however, will not be confined

to

to the mere detail of literary intelligence and chemical conversations; a principal part of their labour will devolve to the practical department of the laboratory. To accomplish this as perfectly as possible, all the interesting discoveries, which from time to time enrich the domain of chemistry, and particularly those complicated, expensive, and difficult experiments, which can be repeated by few individuals only, shall be exhibited in their own laboratory; being persuaded, that important experimental inquiries, when once witnessed, seldom fail to excite that degree of ardour which gives increasing energy to scientific research.

From this the Chemical Society will direct their attention to all such original and specific experiments as may individually be proposed, and the results they afford shall be minuted in the journal of the laboratory kept for that purpose, and afterwards published in such a manner as may be directed. These inquiries will embrace whatever is deemed worthy of experimental research in the extensive departments of philosophical, practical and technical chemistry. It is perhaps needless to state, that their laboratory will be open for the analysis of ores, soils, manures, and such substances in general as are found in the British dominions, and are deemed of private or public importance.

And, as it is certain that the progress as well as the accurate and extensive ideas, which the cultivators of chemical science may acquire, are greatly facilitated and promoted by attending to the manipulations and processes of the practical chemist; it is likewise intended, that all the multifarious operations of the laboratory shall be regularly employed for obtaining from the crude materials of nature, all those substances which the society requires as instruments of research, or as specimens of truths, as well as those articles used in the chemical arts, and by manufacturers and artists. This part of the views of the Chemical Society will constitute a perpetual series of operations, well calculated to exhibit a summary exposition of all the general and particular processes of the scientific laboratory:—a consideration highly important to the progress of real improvement.

To give effect to this undertaking, a regular laboratory is already fitted up, and an extensive collection of apparatus and instruments will be procured, to ensure those auxiliary advantages which are essential to the pursuit of the science.

Such are the outlines of the plan to which the views of the Chemical Society will be directed. A more particular detail of rules and proceedings would be premature and superfluous. It must, nevertheless, be remarked, that whatever encouragement the establishment may receive, the admission of subscribers is, for the present, limited to sixty, and the annual subscription fixed at three guineas.

An unlimited number of gentlemen, residing in the country, may be admitted as subscribers, on paying one guinea annually, which shall entitle them to visit the Society as members whenever they reside in the capital, provided their stay in town does not exceed three months.

After the first meeting the admission of members shall be decided by ballot, and those who are not inclined to adopt the regulations then agreed upon by the majority of the subscribers, shall have their subscription immediately returned.

A code of laws will be formed, and proper officers elected so as to form a regular society, which shall be denominated *The London Chemical Society.*

The admission of members is for the present confined to a committee, who, on the present occasion, address the chemical public, and request, that such gentlemen as are desirous of becoming subscribers may favour them with their names, for which purpose a book is opened at their Laboratory, No. 11, Old Compton-street, Soho.

ROYAL ACADEMY OF SCIENCES, BERLIN.

At a late meeting M. Klaproth read an essay on the chemical properties of the datholith, a fossil newly discovered in Norway: its constituent parts are $36\frac{1}{2}$ of silex, $35\frac{1}{2}$ of lime, and 4 of water.

ACADEMY OF USEFUL KNOWLEDGE AT ERFURT.

At the meeting of the above academy on the 4th of March last, an essay of professor Tromsdorff's was read under the title

title of "New facts tending to increase our knowledge of the chemical nature of Platina."—The author has repeated the experiments of Dr. Wollaston and Mr. Tenant on the above metal, and obtained the same results.

Professor Bernhardi read an essay on the double refraction of gypsum, and has thrown considerable additional light on the subject.

ROYAL ACADEMY OF SCIENCES AT MUNICH.

At the meetings of the above academy, of the 25th of January and 4th March last, M. Ritter proceeded with the account of his experiments on the connexion subsisting between magnetism and electricity: after he has concluded these experiments, he intends giving their results to the public through the medium of some of the foreign journals.

THE NEW RUPTURE SOCIETY, FOR THE RELIEF OF BOTH SEXES AFFLICTED WITH HERNIARY COMPLAINTS AND PROLAPSES.

With a view to interest the public in the success of this institution, it is thought necessary to submit to them the following concise statements, with respect to the nature of those calamities for the relief of which it is intended. The facts here mentioned, it is presumed, will be amply sufficient to excite attention and to induce inquiry.

It has been estimated that at least one person in fifteen is ruptured: but among those classes of the community which are much exposed to laborious employment, the average may be fixed at one in eight or nine. The proportion, however, of sufferers from this disease, is incomparably greater in places of which the situation is low and damp, and the atmosphere relaxing. The committee of this institution, which the public are now requested to patronise, have been credibly informed, that, in some particular parishes, this proportion may be computed at even a fourth of the labouring population!

This complaint is not confined to any particular age or

sex*, nor is it the consequence of depraved habits or immoral behaviour: but it arises from a natural bodily defect, or from very unusual exertions, at any period of life; and can be neither foreseen nor avoided, either by the wealthy or the poor. Its tendency is so alarming, that without timely aid it generally terminates by a painful and rapid dissolution, in early life, or during the vigour of manhood; although, in a majority of cases, its fatal consequences may almost certainly be prevented by the application of an appropriate truss or compress.

Prolapses (a species of disorder vulgarly called *a bearing down of the body*) are extremely similar to ruptures, originating in a similar infirmity of the bodily frame, and requiring a like plan of treatment. They are not, indeed, equally common; but yet occur much more frequently than an inattentive observer might be apt to suppose, and are especially incident to the weaker sex. The idea of delicacy attached to this complaint has the effect of concealing from public view both the number and the sufferings of its victims; circumstances which could not fail, were they fully known, to produce a powerful impression on every humane and benevolent mind. But perhaps the sufferings adverted to will not bear a minute description; and it may therefore suffice generally to observe, that they can hardly be conceived, excepting by those who have had some opportunity of knowing them, either from personal or from professional experience.

* As many persons do not know that females are liable to this heavy affliction, the following statement, founded on actual experience, will show the proportion of herniary complaints in each sex, out of more than three thousand cases.

		Males.		Females:
741 Double Ruptures.	In both thighs (femoral) -	3	&	44
	In both groins (inguinal) -	609	&	85
2272 Single Ruptures.	In one thigh (femoral) - -	57	&	163
	In one groin (inguinal) -	1520	&	399
	In the navel (umbilical) -	36	&	97
	Total	2225	&	788=3013

Of the single ruptures, more than one third happened on the left side, and nearly two thirds on the right side. A very small proportion of triple ruptures, and other extraordinary cases, likewise occurred in the above number; but they were extremely rare, and mostly existed among the female sex.

Among

Among the afflicted poor, where no means of relief have been resorted to during the existence of ruptures and prolapses, the unhappy objects are commonly disabled from fulfilling the ordinary duties of their stations; insomuch that in the army and navy it is an invariable rule to dismiss from the service those men who become ruptured by the violence of their exertions. How lamentable a fact is it, that many thousands of individuals, perhaps during the period of full health and activity, should drag on a most useless and miserable life, for want of either the means or the knowledge requisite to obtain relief, when that knowledge and those means can so easily be provided by their affluent neighbours!

The extreme danger of these complaints, and the great inconveniences entailed on those pitiable beings who survive their misfortune, have given birth to an innumerable host of truss-sellers, rupture-mongers, women-doctors, and ignorant impostors; so that "no disease," as the late celebrated Mr. Pott observes, "has ever furnished such a constant succession of quacks as ruptures have." These pretenders are to be found in almost every city, town, and village throughout this kingdom; for whoever can forge an iron hoop, or fabricate an elastic compress, thinks himself qualified to undertake the cure of an infirmity, the treatment of which demands as much anatomical and surgical knowledge as that of any disorder incident to human nature. If a greater mischief can happen to the ruptured poor than the malady with which they are visited, it is that of unhappily falling into the hands of unprincipled self-taught rupture-doctors and truss-makers, who have no ideas beyond those of money-getting and mechanical action.

The great national importance of relieving the necessitous and afflicted class of people here alluded to, must be sufficiently obvious to every reflecting mind; and, when it is distinctly made known how small a sum will be annually required for the comfort and security of so many deplorable sufferers, no feeling spectator of such wretchedness can withhold his contribution. The expense of procuring a truss, and other incidental means for the relief of a ruptured person, need not, with proper arrangements, exceed the

sum of nine shillings, provided it be a single rupture of the groin or thigh; and in cases of double ruptures, as well as in those which occur near the navel, the expense will seldom exceed eighteen shillings for each patient. The apparatus for prolapses will not, in general, be more expensive than for ruptures.

By adopting a system of œconomy * in the formation and management of a permanent fund for these purposes, a number of subscribers of one or two guineas each, per annum, may be enabled to administer consolation to thousands of their fellow-creatures, who would otherwise die miserably, or live unprofitable members of the community.

One objection, and only one, it is here proper to anticipate, lest some benevolent persons should suppose there is no absolute necessity for the establishment of this institution; namely, "that there already exists a society for these specific objects."

During the year 1796, a society was formed in this metropolis for the sole purpose of gratuitously affording surgical assistance and trusses to ruptured persons, of either sex, in indigent circumstances. The benefits of that establishment were extended to those ruptured soldiers and sailors (properly recommended by their commanding officers) who had been dismissed from his majesty's service; on which account the war-office contributed fifty pounds per annum towards the support of that society. Popular instructions were likewise printed and circulated by the subscribers, for conveying to the afflicted poor such needful information concerning the nature and consequences of ruptures as should lead them to adopt means for their personal safety. But, unfortunately, some circumstances occurred which induced the principal patrons of that institution (after calling a general meeting) to discontinue their sanction, and at length to withdraw from it entirely.

These facts are now too publicly known to require further explanation.

* The surgeon and all the other officers of this new institution, except the collector, serve gratuitously; and there is no expense whatever for house-rent, or committee-rooms, &c. &c.—so that nearly the whole of the funds of this society are exclusively applied to the relief of the afflicted poor.

explanation. It cannot, therefore, be irrelevant to the present appeal in behalf of the ruptured poor to state, that there is ample room to commiserate and improve their condition, notwithstanding former efforts to relieve them. With such design, a new and efficient institution has been recently established for the relief of ruptured persons, which will afford a similar extension of benefits to the army and navy as soon as the funds shall prove adequate for that purpose. At the same time it is resolved, that this new institution shall include, within the sphere of its bounty, a description of sufferers (namely, persons afflicted with prolapses) who have hitherto been left unprotected and forsaken.

The affairs of the New Rupture Society are managed by a committee of governors, consisting of a patron, two or more vice-patrons, one president, twelve or more vice-presidents, two joint treasurers, all the life subscribers of fifteen guineas, and so many yearly subscribers (of not less than two guineas each) as may be approved by a ballot of the said committee, and confirmed at an annual general meeting; among whom are also included the surgeon or surgeons, and secretary or secretaries for the time being, so long as they or either of them do render his or their services gratuitously to this institution.

Life subscribers of fifteen guineas, and contributors of not less than two guineas each per annum, are denominated *governors* of this charity; but subscribers of smaller sums are denominated *members*, and are allowed to recommend patients in the following ratio: viz.

For every guinea subscribed per annum, as well as for every life subscription, two patients (whether afflicted with ruptures or prolapses) may be recommended within the year, to be provided with trusses or bandages not exceeding the expense of nine shillings each; or one patient, requiring a truss or bandage valued at more than nine shillings, and not above eighteen shillings. The same ratio to be observed, and similar advantages extended in the like proportion, to subscribers of larger sums.

Subscriptions for the use of the New Rupture Society are received

received by the following bankers:—Messrs. Hoare, Fleet-street, treasurers to the society; Messrs. Hankey and Co., Fenchurch-street; Messrs. Fuller, Chatteris, and Co., Lombard-street; Messrs. Ransom and Co., Pall Mall; and by Mr. John Sawyer, No. 20, Great James-street, Bedford-row, collector to the society; of whom may be had the printed rules of this institution, and a list of the subscribers, with recommendatory letters for patients, &c. &c.

VACCINE INSTITUTION, BROAD-STREET.

Extract of Minutes of the Vaccine Institution, 44, *Broad-street, Golden-square, June* 1806.

In consequence of adverse reports,

Resolved,

1. That the medical establishment are of opinion, that the cow-pock inoculation, under the regulations of this institution, according to their experience, is decidedly preferable to that of the small-pox.

2. That since the offered reward of five guineas to any person who has been here certified, not one has applied in consequence of a failure.

3. That, in general, patients decline to accept of the certificate, on account of their experience showing the security, after vaccination, on living with persons ill of small-pox.

4. That as in a small proportion persons considered to have been duly vaccinated have taken the small-pox, it is thought proper to grant certificates to those only who have been duly tested at this institution.

5. To give further satisfaction, and considering that the vaccine disorder is a new morbid affection, which requires investigation (one of the great objects of this institution) it is requested that any persons who apprehend that they have taken the small-pox, or have any disorder from vaccination, will apply to the board any Tuesday or Friday at one o'clock, to execute the design of a faithful publication of accumulated evidence.

Persons desirous of promoting the objects of this institution by donation or annual subscription, viz. the inoculating of

of patients, as well as the furnishing of vaccine matter, must apply to No. 44, Broad-street.

The medical establishment consists of Drs. Pearson, Nihell, and Nelson, physicians; Messrs. Keate, Payne, Forster, Heaviside, Gunning, Carpue, and Doratt, surgeons; Messrs. Rivers, Brandé, and De Bruyn, apothecaries.

WM. SANCHO, Secretary.

Dr. Pearson communicated several instances of failures after vaccine inoculation, which, from the respectability of the practitioners under whom they occurred, and the accuracy of their detail, must be admitted. He had been fortunate in not, to his knowledge, having had, in his own practice, a single instance of small-pox after cow-pock; but, from what has of late happened, it would be unjust to ascribe the exemption to any circumstance but an early opinion that the vaccine disorder would require some years to complete its investigation for secure practice, and hence to use precautions not taken by those who represented the subject as exhausted on its first promulgation. It is in vain any longer to impute the failures to the matter taken beyond the appearance of the areola; the use of a rusty lancet; the bursting of the vesicle; the use of "spurious matter"; the susceptibility of taking small-pox more than once, and therefore of small-pox after cow-pock; the using matter "decomposed" by drying on a warmed lancet, or by the heat of sealing-wax melted in sealing a letter containing matter; the inserting the vaccine fluid into a wound deeper than the skin, &c. &c. These circumstances served as bolsters to prop up the practice by those who were either too inexperienced to know, or did not possess candour to own, that experience was wanting to determine the advantages of the new practice. The publications in favour of vaccination are written, for the most part, with the spirit of animosity and not of investigation; of course, they are not calculated to remove doubts, and establish the vaccine practice according to its true value. On the other hand, the adverse writers seem not at all anxious for any thing but to collect and publish,

publish, for the most part, inaccurate accounts of failures, without due regard to the laws of evidence. Neither party seems to be aware that vaccination may leave one out of a thousand still liable to the small-pox, and yet *finally* be rendered to effect the grand objects in view, viz. the extermination of the small-pox, by substituting an affection that in not one case of a hundred is worth calling a disease, and produces death scarcely once in ten thousand cases; for small-pox inoculation, which produces a severe disease in one case of 25, and death in one of 150 or 200 inoculated persons.

XVII. *Intelligence and Miscellaneous Articles.*

SOLAR ECLIPSE.

We have received an account of two observations of the eclipse which took place on the 16th of June. One of the observers, situated in long. 11″,5 east of the royal observatory, saw the beginning at $4^h\ 38^m\ 49^s$ M. T. The other, situated in about long. 1″,0 west, observed the end at $5^h\ 57^m\ 54^s$ M. T. The beginning of the eclipse took place about $2^m\ 27^s$ later than the tables gave it. The end agrees to within about 6″ of the calculated time.

MR. CHENEVIX.

We are happy to inform our readers that the statement of the death of Mr. Chenevix, the celebrated chemist, contained in a foreign journal, is altogether erroneous, as we are well assured that the last accounts from Constantinople announced him to have arrived safe and well in that city.

FOSSIL TOOTH.

At Sir Joseph Banks's *converzatione* on the 22d of June, a very curious and perfect fossil tooth, belonging to an elephant, or to some other huge animal of a former period, was exhibited. It is twelve inches long, near six broad, and of a proportionate thickness. This surprising tooth seems to have belonged to a young animal, as less than one-third of

its

its length projected beyond the jaw. Sir Joseph has had this tooth sawed in two lengthways and polished, by which its internal organization is finely shown. It was stated that this tooth was lately found on the surface of the great clay stratum near Ealing, or Acton, in Middlesex, about 16 feet beneath a bed of gravel.

BEET-ROOT SUGAR.

Late experiments made at Berlin have ascertained that the *yellow* beet-root (*beta lutea*) yields more than double the quantity of sugar that any other species of that vegetable affords.

MISCELLANEOUS.

The following extraordinary event took place on the 4th of May last at Assmanshausen, in the electorate of Mentz:—About three o'clock in the morning the earth was heard to crack with a most dreadful noise: every body was alarmed, but nobody knew the cause. At day-light it was perceived that the high mountain near the village had opened about the breadth of a hand, and about eight o'clock it extended to about seven or eight inches. Between two and three o'clock in the afternoon a considerable part of the mountain fell down, and covered four houses, the inhabitants of which had fortunately escaped.

Owing to the cold on the night of Wednesday, the 14th of May, several hundreds of the swallow tribe, called martins, had clung to the toll table against the turnpike house at Whalley, in Yorkshire: those again had others covering them four or five feet in thickness, all of whom seemed quite in a torpid state. Several dozens were stroked off the board, and those taken up appeared completely lifeless; until about seven o'clock on Thursday morning, when the sun's warmth caused re-animation, and they gradually moved off to the water-side, a distance of about 30 yards. In a short time afterwards they began to skim the surface of the water, and fly with the usual vigour of those birds.

METEOROLOGICAL TABLE,
BY MR. CAREY, OF THE STRAND,
For June 1806.

Days of the Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
May 27	55°	68°	59°	30·00	42°	Cloudy
28	59	74	58	·03	55	Fair
29	60	69	54	29·97	39	Cloudy
30	54	63	48	·80	42	Fair
31	49	56	51	·85	40	Cloudy
June 1	53	61	55	·96	29	Cloudy
2	56	65	55	30·13	38	Fair
3	57	67	55	·10	48	Fair
4	55	60	56	29·70	0	Rain
5	55	69	57	·65	33	Cloudy
6	59	65	56	·85	39	Cloudy
7	55	70	58	30·12	45	Fair
8	61	73	62	·15	43	Fair
9	64	76	67	·19	45	Fair
10	72	81	70	·12	60	Fair
11	70	74	56	·28	75	Fair
12	60	69	58	·50	83	Fair
13	61	75	61	·27	83	Fair
14	64	77	59	·17	81	Fair
15	63	76	61	·25	66	Fair
16	63	75	56	·15	75	Fair
17	55	69	55	·12	66	Fair
18	54	65	52	·22	70	Fair
19	55	68	58	·37	77	Fair
20	58	79	56	·36	101	Fair
21	54	69	56	·13	56	Fair
22	59	66	50	29·90	35	Cloudy
23	59	59	49	30·02	69	Fair
24	50	64	56	·08	70	Fair
25	60	66	57	29·99	49	Cloudy
26	56	58	60	·80	0	Rain

N. B. The barometer's height is taken at noon.

ERRATA.—In the Three last Numbers *Noon* is in the column of *Night*, in the Meteorological Table.—In our last volume, page 287, line 8, for *diameter* read *circumference.*

XVIII. *Letter from Dr.* De Carro *to the Editors of the Bibliothèque Britannique, on the Guinea Worm, and the Sting of the Scorpions of India**.

GENTLEMEN, Vienna, Sept. 24, 1805.

When I last wrote to you on the subject of that celebrated quadruped the shawl goat of India†, I think I informed you that I intended to lay my philosophical friends in Asia under contribution, by requesting communications from them of any thing remarkable in medicine, surgery, or the materia medica. The most zealous and most regular of my correspondents is Dr. Milne, at present residing at Goa, and from him I have received some interesting details upon two of the diseases of India, as communicated by the Rev. Mr. Dubois, the missionary, to Dr. Anderson, physician-general to the government of Madras; and Dr. Anderson's answer; both of which are subjoined.

These observations upon the Guinea worm (*dracunculus*) will be so much the more interesting to your readers, as a short time ago you gave an account of the work of Mr. M'Gregor upon the diseases of the expedition to Egypt ‡. The following details cannot fail to arrest the attention of the public, when we consider that they describe the efficacy of a remedy which seemed to be completely unknown to Mr. M'Gregor and such medical men as have written on the subject:

Letter of the Rev. Mr. Dubois to Dr. Anderson, Physician-general.

DEAR SIR,

The principal object of this letter is to communicate to you two remedies, the efficacy of which I have remarked in a great number of instances, in two diseases very common in this district and in some others: I allude to the disease known among Europeans by the name of the *Guinea worm*,

* From *Bibliothèque Britannique*, vol. xxx.

† See Philosophical Magazine, vol. xxiv. p. 97.

‡ See Philosophical Magazine, vol. xxii. p. 350.

among the Indians by the name of *naramboo* or *nurapoo-chalandy*; and to the pain occasioned by the bite of a scorpion.

These two simple remedies, both of them, I suppose, equally innocent, were communicated to me by a Hindoo physician, who has administered them, with very great success, to a great number of diseased persons who have daily implored his assistance. As this Hindoo was under obligations to me, I requested him to impart these remedies; which he immediately did: and as he enjoined no secrecy upon me, I do not think I shall abuse his confidence in making his communication public.

From the moment I first knew these remedies, I have also had many opportunities of experiencing the efficacy of the one against the Guinea worm in particular, and almost always with success, except in two or three instances, where the persons who had taken the remedy the first time refused to take it a second time; which is often necessary in obstinate cases. But however inveterate the case was, the remedy, upon the second application, forced the departure of the worm, if already formed; or hindered it from forming at all, when taken at the commencement of the disease.

As to the remedy against the bite of scorpions, I have had but few opportunities of trying it, having only administered it three times with equal success. I administered it one hour after the bite, at the very moment the pain was greatest: in half an hour after the pain was greatly diminished, and in an hour it totally ceased.

It is generally known, that the reason why the disease called the Guinea worm is painful and troublesome is because it particularly attacks the legs, often rendering them entirely motionless for many months.

When this disease appears in this and some other districts of the Carnatic and of Madura, it becomes sometimes epidemic to such a degree, that I have seen villages the one-half of the inhabitants of which were attacked at the same time.

Although it appears at all seasons, yet it is much more general

general in the months of December, January, and February; it then rages in several districts at once: at other seasons fewer persons are affected with it.

The symptoms of this malady are almost always the same. A disagreeable sensation is felt for a few days, accompanied by head-ache, pain in the stomach, and nausea; two or three days before the pain is fixed in the place in which the worm appears, an eruption of small pimples takes place, accompanied with a very disagreeable itching; this itching becomes more violent at the place where the worm is about to force a passage, until the pain is entirely fixed to one place. The part then swells, and inflammation soon follows.

In this case, the worm often comes away with the pus, or when the suppuration is upon the point of ceasing; sometimes the place where it is lodged swells like a bladder full of limpid matter, and sometimes only a simple induration is seen, without inflammation.

The period necessary for the discharge of the worm varies much; sometimes it happens at the end of ten days, and sometimes not for two or three months, and during all this time the limb continues in a state of inflammation and suppuration.

Sometimes it comes out all at once, if means are taken to facilitate it, and then it is often alive. I have frequently seen these insects move about for several minutes after coming out of the leg. In general, however, it comes out by little and little. About an inch of it comes out in a day, and it is carefully rolled round a straw, or something else, to hinder it from returning.

If drawn out roughly it breaks, and the piece which remains occasions swelling and inflammation, accompanied by acute pain and followed by suppuration. In this case, the disease becomes very obstinate and very painful.

However severe this disease is, it is never followed by gangrene; it sometimes happens, however, that the principal nerves are so injured that the limb becomes short or deformed.

These worms are of different sizes; some of them I saw

were a foot and a half long, although they are commonly shorter; but in general their size and shape may be comared to the small string of a violin.

It is at the extremities, particularly the legs, that they appear, and sometimes in the arms. Some Hindoos assured me that they had seen them in the nose, the ears, the eyebrows, &c.; but I have never seen them except in the legs and arms.

I do not feel myself capable to divine the origin and causes of this singular disease. The Hindoos, accustomed to attribute the most of their diseases to the bad qualities of the water they use, generally believe that element to be the cause; pretending the worms are there engendered, and, being swallowed, that they increase in the human body until they find an outlet. According to this system, it would be very difficult to explain how these insects could pass through the organs of deglutition and digestion in a state of life. On the other hand, admitting that water has no part in their formation, it will be difficult to explain it. 1st, How does it happen that the inhabitants of a village who drink the water from the same well are attacked with this disease, while others, at the distance of only half a mile, are not attacked? 2d, How does it happen that those who live at the distance of a mile from a river, and who are obliged to drink well water, are almost regularly every year attacked with the above?

The climate also may have some effect upon this malady. It rarely attacks those who live near the sea-shore. I never saw it in any part of the Mysore, and it is even not known by name beyond the Ghauts; while it rages continually in the Carnatic, and other districts within the Ghauts, the neighbourhood of rivers excepted, as I have already remarked.

But whatever is the cause and origin of this disease, I shall be happy if the remedy I propose be a specific against it. The following example will at least prove its efficacy:

A long time before this remedy was known to me, I was told that the Bramins were never, or at least very rarely, attacked with the above disease, although living in villages the inhabitants

inhabitants of which suffered constantly from it every year. I paid no attention to this observation, regarding it as one of those assertions without foundation so common among the Hindoos, who consider this exemption as belonging to the sacred character of this class of men. Reflecting afterwards upon this assertion, after knowing the remedy, I explained it by recurring to the quantity of assafœtida which the Bramins daily and constantly use in their victuals as a seasoning; and this also is the principal ingredient of the remedy.

The remedy employed against the bite of a scorpion may be applied with the same success against the sting of winged insects, according to the following recipe:—The shrub, the root of which is a specific in this case, is called by the Hindoos *kaletchy-chaddy*. Being ignorant of the name given to this shrub by European botanists, I send you two specimens of the fruit, which are called *kaletchy-kabye*, in order that you may ascertain the shrub which produced them. They are found of different hues; but, as there are various species of them, I have to caution you to choose such as have their fruit of a white colour, because the root of the other kinds has not the same virtue.

Remedy against the Bite of Scorpions.

Take, at the new moon, the root of the shrub called kaletchy-chaddy, of the kind which produces white fruit; cut them into pieces of three or four inches long, and allow them to dry in the shade.

When a person has been bitten by scorpions, cut off a piece of root as large as a bean; let the patient place it in his mouth, pressing it gently with his teeth; he must swallow his saliva, taking a new piece of the root every ten minutes. At the same time pulverize a small quantity of the root, upon which throw some drops of water; and, after having made a paste, apply it to the wound.

Remedy against the Guinea Worm.

1st, Take of good assafœtida, called by the Tamuls *peroongahyam*, as much as will weigh seven *fanams* of gold (about three-fourths of a *ragoda*).

 2d, The

2d, The fruit, well known in all the gardens of India, called by the Tamuls *katricahe,* and by the Portuguese *beringelle.*

3d, Oil of sesamum, called by the Tamuls *nalla-yennie,* in sufficient quantity to fry the *katricahe* or *beringelle.* Bruise the assafœtida, and, after having divided the beringelle into three equal parts with a knife, insert one-third of the assafœtida in each portion of the fruit: after having tied them with a thread, fry the beringelle in the oil of sesamum in a convenient vessel; let the patient take one portion of the remedy upon going to bed, another next morning, and the third upon going to bed the second night; rub the part of the body where the worm is lodged with the oil which fried the fruit containing the assafœtida, for three days, and three times a-day.

If this remedy is resorted to at the commencement of the malady, it will arrest its progress and hinder the formation of the worm; if made use of after the worm is formed, it will soon bring it away; and in every case the pain ceases in three or four days, unless the disease is very obstinate, when the remedy must be repeated, and it never fails on the second application.

Dr. Anderson's Answer to the Rev. Mr. Dubois.

DEAR SIR,

I have read your letter with great pleasure and attention, because I do not know that any thing similar has been published in the English language; and, in fact, this Guinea worm, or *dracunculus,* which you have described so exactly, is one of the most dreadful maladies to which the inhabitants of this great peninsula are subject.

What you have said upon the Hindoostanee names of these remedies, &c. is of real importance to Europeans, as enabling them to procure them from the natives when they are in want of them.

As the knowledge of these Tamul names, however, will be only useful in India, for the instruction of the naturalists of Europe, I have to observe, that the *kaletchy,* which you recommend as a remedy against the bite of winged

 insects,

insects, is, according to Linnæus, of the order of *Lomentaceæ* and of the genus *Guilandina*. Botanists have distinguished two species, the *bonduc* and the *bonduella*. They have made no observation upon the colour of the seeds; but I saw a lady of Bengal, on her way to Europe, who gave her child, when attacked by an intermittent fever, these seeds of a clear brown colour, in preference to Peruvian bark.

The reason you assign why the Bramins are less subject to the Guinea worm than the other natives, led you to a very natural conclusion. I must say, however, that I have remarked the same difference in the susceptibility of our officers and that of the common soldiers, having often thirty of the latter under my care at once, and not one of the former. Not observing any difference in their food which might lead me to the same conclusion with yourself, I thought these soldiers contracted the above malady by sleeping on brick pavements, as was once the custom, and that the remedy really was to make them lie upon wooden benches, which are now erected in all our barracks and guardhouses.

We know well that the *ichneumon* fly lodges its eggs in the silk-worm wherever it can find a corner convenient enough for its future subsistence; and we see worms every day even in the kernels of fruits, which had been deposited as an egg, without the least exterior sign: we cannot allege, with propriety, that worms enter into the bodies of animals by the organs of deglutition or digestion, when we see them in the eyes of horses and the livers of sheep.

I am inclined to believe that the Guinea worm is introduced into the human body in the same way as various other insects which deposit their progeny there, or in the state of an egg it is sucked up from the humid earth by means of the absorbents of the skin. Whatever it may be, it is our duty to do our endeavours to destroy it; and, as assafœtida is known over all the world, recourse may be had to it both as a preservative and a cure.

In my whole practice I never found any thing so efficacious as cataplasms made of *cattale* or *aloe littoralis*; a remedy which was communicated to me by a Hindoo. The

 saponaceous

saponaceous quality which it appears to possess softens the teguments when strongly inflamed, diminishes the tendency to gangrene, and favours the coming out of the worm; which I have seen sometimes lodged in the pectoral muscles as well as in the inferior extremities.

If you reflect, that insects spread themselves in clouds over districts where we never saw them before, you will find it very difficult to ascertain the local situation of certain species, such as those known by the name of the Guinea worm, while our own vessels alone frequent the mouths of some rivers in Africa, whence we first learned the knowledge of them. Rajamundry, which is situated on the banks of the Codavery, the largest river on the Malabar coast, is very subject to it: we know also that it is very common at Trichinopoly, upon the river Cavery; and even at Bombay, which is a sea-port. It appears to me, besides, that the circumstance of their being so common in your district, which abounds so much in brackish water, renders the idea probable that the vitriolic and muriatic salts, which are found in wells and in the sea, favour the propagation of these worms, although I have seen them in places where the purest spring water only is drunk.

Note by Dr. De Carro.

The Guinea worm not being a disease to which Europeans are subject, the remedies recommended by Mr. Dubois can only be used in the case of worms in the intestinal canal, where the ordinary remedies have not succeeded; and even the employment of assafœtida as a vermifuge is no novelty. But I invite agriculturists to try whether the above remedies may not be applied to sheep, and such animals as are subject to worms in various parts of their bodies.

XIX. *Che-*

XIX. *Chemical Experiments on Guaiacum.* By Mr. William Brande*.

Among the numerous substances which are comprehended under the name of resins, there is perhaps no one which possesses so many curious properties as that now under consideration; and it is remarkable that no more attention has been paid to the subject, since many of the alterations which it undergoes when treated with different solvents have been mentioned by various authors.

§ I.

Guaiacum has a green hue externally; is in some degree transparent; and breaks with a vitreous fracture.

When pulverised it is of a gray colour, but gradually becomes greenish on exposure to air.

It melts when heated, and diffuses at the same time a pungent aromatic odour.

It has when in powder a pleasant balsamic smell, but scarcely any taste, although when swallowed it excites a very powerful burning sensation in the throat.

Its specific gravity is 1·2289.

§ II.

1. When pulverised guaiacum is digested in a moderate heat with distilled water, an opaque solution is formed, which becomes clear on passing the whole through a filter.

The filtrated liquor is of a greenish-brown colour; it has a peculiar smell, and a sweetish taste.

It leaves on evaporation a brown substance, which is soluble in alcohol, nearly soluble in boiling water, and very little acted upon by sulphuric ether.

This solution was examined by the following re-agents:

Muriate of alumina occasioned a brown insoluble precipitate after some hours had elapsed.

Muriate of tin formed a brown flaky precipitate under the same circumstances.

Nitrate of silver gave a copious brown precipitate.

* From the *Transactions of the Royal Society for* 1806.

Suspecting

Suspecting the presence of lime in the solution, I added a few drops of oxalate of ammonia, when the liquid immediately became turbid, and deposited brown flakes, which, after having been treated with boiling alcohol, yielded traces of oxalate of lime.

These effects, therefore, indicate the presence of a substance in guaiacum which possesses the properties of extract *; the action of the re-agent is, however, somewhat modified by a small quantity of lime which is also in solution.

One hundred grains of guaiacum yielded about nine grains of this impure extractive matter.

2. Alcohol dissolves guaiacum with facility, leaving some extraneous matter, which generally amounts to about five per cent.

This solution is of a deep brown colour; the addition of water separates the resin, forming a milky fluid which passes the filter.

Acids produce the following changes:

A. Muriatic acid throws down an ash-coloured precipitate, which is not redissolved by heating the mixture. In this case the resin appears but little altered.

B. Liquid oxymuriatic acid, when poured into this solution, forms a precipitate of a very beautiful pale blue colour, which may be preserved unaltered.

C. Sulphuric acid, when not added in too large a quantity, separates the resin of a pale green colour.

D. Acetic acid does not form any precipitate. This acid is, indeed, capable of dissolving most of the resins.

E. Nitric acid, diluted with one-fourth of its weight of water, causes no precipitate till after the period of some hours. The liquid at first assumes a green colour, and if water be added at this period, a green precipitate may be obtained: the green colour soon changes to blue, (when, by the same means, a blue precipitate may be obtained;) it then becomes brown, and a brown precipitate spontaneously

* By the term *extract*, I mean that substance which by chemists is called the extractive principle of vegetables. *Vide* Thomson's System of Chemistry, 2d edit. vol. iv. p. 276.

makes

makes its appearance, the properties of which will be afterwards mentioned.

The changes of colour produced by nitric and oxymuriatic acids in the alcoholic solution are very remarkable, and, I believe, peculiar to guaiacum: there is, moreover, much reason to suppose that the above alterations in colour are occasioned by oxygen *. It likewise appears from that which has been stated, that the blue and green oxides (if they may be so called by way of distinction) are soluble in the mixture of nitric acid and alcohol, while the brown precipitate is insoluble.

F. Alkalis do not form any precipitate when added to the solution of guaiacum in alcohol.

3. Guaiacum is less soluble in sulphuric ether than in alcohol: the properties of this solution nearly coincide with those just mentioned.

4. Muriatic acid dissolves a small portion of guaiacum, the solution assuming a deep brown colour; but if heat be applied, the resin melts into a blackish mass, preventing any further action from taking place.

5. Sulphuric acid forms with guaiacum a deep red liquid, which, when fresh prepared, deposits a lilac-coloured precipitate on the addition of water: a precipitate is also formed by the alkalis. If heat be employed in forming this solu-

* The following experiments appear to verify this supposition:

Fifty grains of freshly pulverised guaiacum were introduced into a glass jar containing 60 cubic inches of oxymuriatic acid gas. The resin speedily assumed a brown colour, having passed through several shades of green and blue. Liquid ammonia was poured on this brown substance while yet immersed in the acid; the whole became green; it therefore seemed thus to be deprived of part of the oxygen which it apparently had acquired by the preceding experiment. An equal portion of the same guaiacum was exposed, under similar circumstances, to the action of oxymuriatic acid, excepting that the glass in which the experiment was made was covered with a black varnish, and placed in a dark apartment. On examining the result of this experiment, the resin was found to have undergone precisely the same changes as when exposed to light. Ammonia had also the same effect.

Guaiacum was also exposed over mercury to oxygen gas; the resin assumed, after some days, the green colour which a longer exposure to the atmosphere produces: this change was likewise found, by a second experiment, to be effected without the presence of light.

tion,

tion, the resin is speedily decomposed; and, if the whole of the acid be evaporated, there remains a black coaly substance, together with some sulphate of lime.

6. Nitric acid appears to exert a more powerful action on guaiacum than on any of the resinous bodies.

One hundred grains of pure guaiacum, previously reduced to powder, were cautiously added to two ounces of nitric acid, of the specific gravity of 1·39. The resin at first assumed a dark green colour, a violent effervescence was produced, attended with the emission of much nitrous gas, and the whole was dissolved without the assistance of heat; which is not the case with the resins in general; for, when these bodies are thus treated with nitric acid, they are commonly converted into an orange-coloured porous mass.

The solution thus formed yielded, while recent, a brown precipitate with the alkalis, which was redissolved on the application of heat, forming a deep brown liquid.

Muriatic acid also separated the guaiacum from this solution; not, however, without having undergone some change.

Sulphuric acid caused no precipitate.

After this solution of guaiacum in nitric acid had remained undisturbed for some hours, a considerable proportion of crystallized oxalic acid was deposited.

When guaiacum was treated with dilute nitric acid, the results were somewhat different. A slight effervescence took place, and part of the resin was dissolved, the remainder being converted into a brown substance resembling the precipitate obtained from the alcoholic solution as above mentioned (2. E.).

This brown substance appears to be guaiacum, the properties of which are materially altered by its combination with oxygen; and I am led to think that the changes of colour produced by nitric and oxymuriatic acids are the consequence of the different proportions of oxygen with which the guaiacum has been united; for we know that the colours of metallic and many other bodies are greatly influenced by the same cause.

The brown substance was separated by filtration; the filtrated

trated liquor yielded yellow flocculent precipitates with the alkalis, and on examination was found to hold nitrate of lime in solution.

The undissolved portion was of a deep chocolate brown colour. A similar substance may also be obtained by evaporating the recent nitric solution to dryness, taking care not to apply too much heat towards the end of the process.

The substance obtained by either of these means possesses the properties of a resin in greater perfection than guaiacum; it is equally soluble in alcohol and sulphuric ether, insoluble in water, &c.; but when burned it emits a peculiar smell, more resembling animal than vegetable bodies. If, however, fresh portions of nitric acid be added three or four times successively, or if a large quantity be employed to form the solution, the product obtained by evaporation is then of a very different nature; for it has lost all the characteristic properties of a resin, having become equally soluble in water and alcohol; the solution of it in this state having an astringent bitter taste *.

7. Guaiacum is copiously soluble in the pure and carbonated alkalis, forming greenish brown liquids.

Two ounces of a saturated solution of caustic potash took up rather more than 65 grains of the resin; the same quantity of liquid ammonia dissolved only 25 grains.

Nitric acid formed in these solutions a deep brown precipitate, the shades of which varied according to the quantity of acid which had been employed.

This precipitate was found, on examination, to possess the properties of that formed by nitric acid in the solution of guaiacum (2. E.) in alcohol.

Dilute sulphuric acid, when poured into any of the above alkaline solutions, formed a flesh-coloured curdy precipitate. Muriatic acid produced the same effect.

The two last-mentioned precipitates differ from guaiacum, in being less acted upon by sulphuric ether and more soluble

* *Vide* Mr. Hatchett's two papers on an artificial Substance which possesses the principal characteristic Properties of Tannin. Phil. Trans. 1805, p. 211 and 285.

in

in boiling water; their properties, therefore, approach nearer to extract. Moreover, when these precipitates were redissolved in ammonia, and were again separated by muriatic acid, the above-mentioned properties became more evident.

§ III.

One hundred grains of very pure guaiacum, in powder, were put into a glass retort, to which the usual apparatus was adapted. The distillation was gradually performed on an open fire, until the bottom of the retort became red hot. The following products were obtained:

	Grains.
Acidulated water - - -	5·5
Thick brown oil, becoming turbid on cooling	24·5
Thin empyreumatic oil - -	30·0
Coal remaining in the retort - -	30·5
Mixed gases, consisting chiefly of carbonic acid and carbonated hydrogen - -	9·0
	99·5

The coal, amounting to 30·5 grains, yielded, on incineration, 3 grains of lime. To discover whether any fixed alkali was present, 200 grains of the purest guaiacum (that in drops) were reduced to ashes; these were dissolved in muriatic acid, and precipitated by ammonia; the whole was then filtrated, and the clear liquor evaporated to dryness; but not any trace of a neutral salt, with a basis of fixed alkali, was perceptible.

§ IV.

From the action of different solvents on guaiacum, it appears, that, although this substance possesses many properties in common with resinous bodies, it nevertheless differs from them in the following particulars:

1. By affording a portion of vegetable extract.

2. By the curious alterations which it undergoes when subjected to the action of bodies which readily communicate oxygen, such as nitric and oxymuriatic acids; and the rapidity with which it dissolves in the former.

3. By

3. By being converted into a more perfect resin, in which respect guaiacum bears some resemblance to the green resin which constitutes the colouring matter of the leaves of trees, &c.*

4. By yielding oxalic acid.

5. By the quantity of charcoal and lime which are obtained from it when subjected to destructive distillation.

§ V.

From the whole, therefore, of the above-mentioned properties, it evidently appears that guaiacum is a substance very different from those which are denominated resins, and that it is also different from all those which are enumerated amongst the balsams, gum resins, gums, and extracts: most probably it is a substance distinct in its nature from any of the above, in consequence of certain peculiarities in the proportions and chemical combination of its constituent elementary principles; but as this opinion may be thought not sufficiently supported by the facts which have been adduced, we may for the present be allowed to regard guaiacum as composed of a resin modified by the vegetable extractive principle, and as such, perhaps the definition of it by the term of an *extracto-resin* may be adopted without impropriety.

P. S. I have observed that the action of oxygen on some of the other resinous bodies is very remarkable. It is well known, that by digesting mastich in alcohol a partial solution only is formed, and there remains an elastic substance, which is generally said to possess the properties of pure caoutchouc: it appears, however, to differ from this substance, in becoming hard when dried by exposure to air. Moreover, I have remarked that the part of mastich which remains dissolved by alcohol, may be again precipitated by water, and, on examination, I found the precipitate to pos-

* This substance was found by Proust to be insoluble in water, and soluble in alcohol. When treated with oxymuriatic acid it assumed the colour of a withered leaf, acquiring the resinous properties in greater perfection.—*Vide* Thomson's System of Chemistry, 2d edit. vol. iv. p. 318.

sess the properties of a pure resin: but when a stream of oxymuriatic acid gas was made to pass through the solution, a tough elastic substance was thrown down, which became brittle when dried, and was soluble in boiling alcohol, but separated again as the solution cooled: its properties, therefore, somewhat approached to those of the original insoluble part.

XX. *Natural History of the domestic Pigeons of Spain, particularly in the Province of Valentia. By Don* Antonio Joseph Cavanilles*.

However easy it is to distinguish the generic character of pigeons, it is very difficult to discover the distinguishing marks of each class. This arises from the frequent crossing of the breeds, particularly among domestic pigeons, which has almost entirely destroyed their primitive forms. From this arise the singular mistakes of many naturalists, of which I shall take no further notice. The following are such observations as I have been able to collect, particularly in the province of Valentia, during a long series of years. I begin by my division of the races of domestic pigeons.

Generic Characters.

A straight conical beak; at the bottom of the upper part two small protuberances; the point a little bent. The nostrils oblong, half covered with skin, and situated obliquely on the side of the axis of the beak. The tongue entire. At each foot four toes without any membrane, divided almost to the bottom; three before, and the fourth behind.

Observation.

Many naturalists add to the generic characters of pigeons that of having the feet furnished with feathers; but, as

* From *Bibliothèque Physico-Economique*, no. 3, an. 1805; edited by Sonnini.

hardly

hardly any domestic pigeon has the feet feathered, I omit this character as superfluous *.

First Division.

Character.—A rapid and continued flight; which, however, is sometimes more and sometimes less rapid.

First Sub-division.

Character.—The twelve feathers of the tail of a different colour from those of the body.

Classes.

1. Body white; tail black. In Spanish, *paloma colinegra*: in the language of Valentia, *colom culinegre*.

2. Body white; tail of a blueish gray. Span. *paloma coliazul*: Val. *colom culisendros*.

3. Body white; tail red. Span. *coliroxa*; or *colibaya*, according as the feathers of the tail are of a bright red or of a paler red. Val. *culliroig*.

4. Body black; tail white. Span. *coliblanca de negro*. Val. *culiblanch de negre*.

5. Body red; tail white. Span. *coliblanca de roxo*. Val. *culiblanch de roig*.

6. Body gray; wings streaked; back spotted; tail white. Span. *coliblanca de goteado*: Val. *culiblanch de gotat*.

7. Body ash-coloured; wings streaked; back white as snow; feathers white. Span. *coliblanca de nevada*: Val. *culiblanch de nevát*.

Second Sub-division.

Character.—The principal feathers of the tail of a different colour from those of the body.

Classes.

1. Body white; feathers black. Span. *alinegra*: Val. *alinegre*.

* I do not know any naturalist who has presented as one of the generic characters of pigeons, that of having the feet covered with feathers. It is probably only in Spain that they have fallen into this error; but M. Cavanilles was mistaken when he wrote that this attribute of feathers at the feet did not belong to almost any domestic pigeon. In fact, it is known that there are many of them rough-footed.—*Note of the French editor.*

2. Body black; feathers white. Span. *aliblanca de negro.* Val. *aliblanch de negre.*

3. Body red; feathers white. Span. *aliblanca de roxo.* Val. *aliblanch de roig.*

4. Body ash-coloured; back speckled; feathers of the tail white. Span. *aliblanca de goteado.* Val. *aliblanch de gotat.*

Third Sub-division.

Character.—Tail, feathers of the wings, neck and head, of a different colour from those of the body.

Classes.

1. Body black; tail, feathers of the wings, neck and head, white. Span. *mongin de negro:* Val. *mongi de negre.*

2. Body red; tail, feathers of the wings, &c. white. Span. *mongin de roxo:* Val. *mongi de roig.*

3. Body ash-coloured; wings streaked with white; back speckled; tail, feathers of the wings, &c. white. Span. *mongin de goteado:* Val. *mongi de gotat.*

4. Back blueish gray; tail, feathers of the wings, &c. white. Span. *mongin de azul:* Val. *mongi de cendrós.*

Observation.

By mixing the fifteen races which we have just given an account of, pigeons might be obtained of the most magnificent and varied colours, which in Spain are generally named *figuras,* and in the language of Valentia *figures.*

Fourth Sub-division.

Character.—Wings streaked; the eyes surrounded with a flesh-coloured membrane.

Classes.

1. Body ash-coloured gray; back spotted, streaked with a brownish red. Span. and Val. *gris.*

2. Body ash-coloured; beak speckled; wings streaked; tail terminated with black. Span. *goteada:* Val. *gotat.*

3. Body ash-coloured, spotted with black; wings streaked of the same colour; tail terminated with black. Span. *prieta,* or *gargada:* Val. *carregát.*

4. Body

4. Body ash-coloured gray; back white as snow; throat, head, and neck, of a bright green. Span. *nevada:* Val. *nevat*.

5. Body blueish gray; wings streaked with black; tail terminated with black. Span. *azul de la raza:* Val. *de la rasa* (*columba tabellaria* Linn.*).

6. Beak short, with large protuberances on the upper part; no circular skin about the eyes. Span. *paloma de casta,* or *ladrona:* Val. *ladu*.

7. The crop, which the bird inflates to the size of its body. Span. *buchona:* Val. *pitimflat* (*columba gutturosa* Linn.†).

8. A pigeon which rises and falls by circular and almost perpendicular movements. Span. *paloma volteadora.* Val. *colom refilador* (*columba gyratrix* Linn.‡).

9. The eyes surrounded with a flesh-coloured skin; beak short, with large protuberances. Span. *flamenquilla:* Val. *colom d'ull*.

Second Division.

Character.—Short flight; sometimes heavy.

First Sub-division.

Character.—Small body; heavy flight.

Classes.

1. Feathers curled. Span. *rigeda:* Val. *risat* (*columba hispida* Linn.§).

2. Feathers of the tail rising and spreading out like those of the peacock, from the number of eight to eighteen ‖. Span. *colissava:* Val. *calissavo* (*columba laticauda* Linn.)

Second Sub-division.

Character.—Body large; unwieldy; heavy; flight short.

* This is the messenger pigeon.

† The large throated pigeon. There are many varieties of them known.

‡ It is generally called the *tumbling pigeon,* and sometimes the *pantomime pigeon,* because its movements and its tumblings have made it be compared to those of the vaulters.

§ This class is that of the curled pigeon; it is entirely white, and all the feathers are curled.

‖ The most beautiful pigeons of this class have to the number of 36 feathers in the tail. This is the *peacock pigeon.*

Classes.

1. A red skin about the eyes; a large tubercle upon the beak. Span. *paloma flamenca.* Val. *peter d'ull* (*columba turcica* Linn.*).

2. No bare skin surrounding the eyes; beak long. Span. *paloma comun grandi:* Val. *peter* (*col. hispan.* Linn.).

Third Sub-division.

Character.—A middle-sized body; a more bold and more vigorous flight than that of the preceding.

Classes.

1. The feet feathered to the nails. Span. *paloma comun calzada:* Val. *roquer calsat* (*columba dasypus* Linn.).

2. The feet destitute of feathers. Span. *paloma comun:* Val. *roquér.*

Explanations.

The pigeons comprised in the preceding division form a part of the first division of Linnæus (*cauda æquali*), which includes the pigeons which have the feathers of the tail of an equal length. In the first division we find a great many classes well characterized, because amateurs have taken great care to preserve them unmixed: on this account, however, their broods are less abundant. The most beautiful, and the most numerous, are, undoubtedly, those of the first sub-division.

First Division.

First Sub-division.

The pigeons comprised in this first division are nearly of the same size as the wild pigeons. Span. *paloma campesina.* They have oval heads, less lengthened towards the beak than the former; the beak short, conical, with very small tubercles upon the beak; the eyes brilliant, almost always surrounded with a bare flesh-coloured skin; the feet not furnished with feathers. Many of them have a kind of small crown, but the feathers of which, in place of falling one over another, support each other towards the hinder part of

* This is the *Turkish pigeon*, a very scarce breed, and whose colours are variegated.

the

the head: in short, on most of those pigeons is observed a row of curled feathers from the lower part of the beak to the bottom of the throat, which in Spanish is called *repelon* or *chorerra*, and in the language of Valentia *venera* *.

Besides, the pigeons of this sub-division are not the most fruitful, nor the strongest in flight; they are, however, excellent hatchers, and always preserve their original beauty.

The pigeons of classes 1, 2, 3, 4, 5, of this sub-division have never more than two colours, one of which extends over the whole body, and the other only upon the twelve feathers of the tail. Those of classes 6 and 7 have, on the contrary, different colours: white always on the tail; brown or blackish on the wings; blueish green upon the throat, head, and neck; and gray or ash-coloured on the other parts of the body. The pigeons of the second sub-division of the first division have uniformly the head lengthened without any tuft. They have only the row of curled feathers to the bottom of the neck: otherwise they resemble those of the first sub-division.

The particular character of this sub-division consists not only in the large feathers of the wings, which are of a different colour from those of the body, but principally in the determinate number of those feathers; that is to say, three in one wing, and two in the other.

Pigeons are sometimes found of this sub-division which have only two large feathers in each wing, and which, on account of their scarcity, are highly esteemed.

As to the pigeons of the third sub-division in the first division, they have always fifteen white feathers in the wings; that is to say, seven in one wing, and eight in the other. If, however, there should some be found with more, which is what very seldom happens; for instance, eight in one wing and nine in the other, they are held in the highest estimation †. They are a little smaller than those of the preceding

* Linnæus has formed a particular class of pigeons, which he has designed under the denominations of *columba cucullata* and *columba turbita*; but I have every reason for departing from his opinion, because the characters which he assigns them are not exclusive.—*Note of the French Editor.*

† This observation upon the difference of the number of feathers which

 compose

preceding sub-division; their beak is a little more lengthened; they have not, however, either the curled feathers on the neck, nor the tuft on the head *.

The fourth sub-division of the first division comprehends the numerous classes of pigeons called *columbas pasdas y azules* (*columba domestica* Linn.), which have the greatest resemblance to the wild pigeons; and they are also the most fruitful, and the most powerful in flight. All, except those which are named *flamenquillas*, No. 9, are of a greenish blue, more or less clouded on the neck, breast, and head; the beak is lengthened; the feathers of the wings have at the ends blackish spots; the rest of the wings is brownish, or black. The colour of the back varies very much. In some it is ash-coloured, almost white, without the least spot: in others it is covered with small round black spots: many have it lead-coloured, often very deep, without any mixture; but it is frequently covered with ash-coloured spots.

All the pigeons of this sub-division can inflate their crops; but those which are most distinguished on this account are the *buchonas*, No. 7.

They can inflate the crop to the size of their bodies in such a manner, that it hangs before them like a ball. As to the *volteadores*, No. 8, that sort of stupidity with which they allow themselves to fall often in a straight line on the house-tops, &c., is not certainly a mark of slavery, as Buffon imagines, for in the province of Valentia they enjoy their full liberty.

As to the *azules de rasa*, No. 5. (*columba tabellaria* Linn. †), they are made use of here as well as in Asia as messengers; but they are much oftener the subject of bets and wagers, and pigeon races, for which they are well qualified, from their attachment to their native place, or their ordinary place of residence.

compose each wing of the same bird is very singular: no ornithologist has made it. I intend to prove it on the pigeons of our country.—*Note of the French Editor.*

* The Dutch shell pigeon (*columba galenta* of Linn.) is of this number.—*Note of the above.*

† The *messenger pigeon.*

The

The *ladronas* or *palomas de casta,* No. 6, have the head a little lengthened, with tubercles a little more considerable upon the beak; but otherwise they bear a great resemblance to the *rasa* pigeons; like them they testify a strong attachment for the place of their abode, and the same ardent desire of coupling themselves. They make use of those two classes for alluring wild pigeons. They are taken in the following manner:

In the beginning of November they separate all the males from the females, with which they had been paired from the beginning of May. Scarcely do they see themselves abandoned by their mates, when they fly in flocks towards the steeples, where the wild pigeons generally build their nests; they attract in this manner the young males who have no companions, and lead them to their pigeon-houses: sometimes, however, the males appear to be apprehensive of danger, and return when they are arrived at the entrance of the pigeon-house. They can at pleasure exercise this kind of piracy, whether by males or by females, according as they separate the one from the other.

The flamenquillas, No. 9, are distinguished from all the other classes by their plumage, which varies much on the different individuals. There are some entirely black, others are white, and some again are so mixed with various colours that it is impossible to fix on any of them. The eyes are encircled with a flesh-coloured skin; the head is less lengthened; the beak short, with large protuberances: otherwise they are, if we may so term them, the transition to the second division, from which they differ by their strong and rapid flight. We will now speak of that second division.

Second Division.

We ought here to observe, that, in general, all the classes of this division are held in little esteem (except, however, those of the first sub-division), but on account of their fecundity, and the savour of their flesh. Amateurs have taken much less pains to preserve their original forms, and all the classes are permitted to unite and mix together at will.

First Sub-division.

The character of the class No. 1. is known by its denomination, as the feathers are curled and elevated, and not smooth and joined together like those of the other classes, which occasions their flight to be extremely difficult and troublesome, because the air passes through their wings. We find in this class, in regard to size, colour, and quality, very great differences. There are some white, which are a little larger than turtle doves, and which, on account of their separated and curled feathers, are absolutely incapable of flying: there are some of variegated colours, nearly of the same size as wild pigeons, having a crest, and the feathers less curled, &c. In general, they are reared rather on account of their singularity than their utility, because their broods seldom succeed. Amateurs are in the habit of covering their eggs with other pigeons.

In regard to the *colipavas*, No. 2, the most esteemed are those which have 36 feathers in the tail, although they also prize those which have but 34 and 28; but those that have less are held in no consideration. This large tail embarrasses their flight a little; it gives them, however, a magnificent aspect when they make love, and are in their pride. In this situation they extend it round towards the front like peacocks, and, lengthening the neck, they join their head to it, and often remain a minute in this forced attitude, during which all their body undergoes a tremulous motion*. Their colours are much variegated, and their young rarely succeed.

Second Sub-division.

The two classes, Nos. 1 and 2, of this sub-division, are clumsy, and almost as large as common hens.

They vary partly by the large or small tubercles of the beak, and partly by the existence or the want of the flesh-coloured skin which surrounds the eyes. They possess an infinite variety of colours; they are extremely fruitful; and it is remarked, that the young take more frequently the co-

* Thence comes it that they also give to the *peacock pigeon* the name of the *trembling pigeon.—Note of the French Editor.*

lour

lour not of the immediate generation but that of the preceding one.

Third Sub-division.

The classes Nos. 1 and 2. are still more productive than No. 2. of the preceding sub-division; they hold, as to *their* size, the middle place betwixt the *flamencas* and the wild pigeons.

The *calzadas*, or the class No. 1, have alone the inconveniency of their feet often filling with ordure in such a manner that they frequently break their eggs.

This is all I have to say upon the division of pigeons. I shall yet add some general remarks upon their education in this country, and upon some peculiarities of their habits.

General Remarks.

It is customary in Spain, and particularly in the province of Valentia, to rear a prodigious number of domestic pigeons. In this latter province a pigeon-house is seen on almost all of the flat-roofed houses. These are little square turrets, of different heights and breadths; they are generally formed by joining joists together, the interstices of which are filled with rush mats; and the bottom, where the pigeon-holes are, is laid over with small thin squares of stone ware. The fecundity of the pigeons in this country is extraordinary. They begin to produce, according to the classes, in the fourth, fifth, or sixth month *; and continue thus until their twelfth, fourteenth, and even until their twentieth year.

In general, they bring up from 22 to 24 couples of young. They lay two eggs in less than 24 hours. In spring and in summer they only sit 15 or 16 days; in autumn and in winter, 20 or 21 days. This great vigour and activity of production must necessarily be attributed to the mildness of the climate, and to the choice and abundant nourishment. However, the neatness and good situation of the pigeon-houses have also a very great influence.

* In winter they begin a month later. Sometimes, but seldom, the *palomas padas* commence in the third month.—*Note of the French Editor.*

What

What Buffon and other naturalists say of the manner in which pigeons hatch, does not appear to me to be correct; for the female, according to observations pursued during a great many years, sits from three o'clock in the afternoon until half past ten next morning; the male then relieves her, and sits from that time until three in the afternoon.

I can no longer admit the great encomiums which Buffon has bestowed upon the invariable fidelity of pigeons. Birds, males as well as females, but the latter less frequently than the former, seek for stolen pleasures even during the time of hatching; at least, they certainly do not allow any such occasions to escape when they occur. I have made this observation an infinite number of times: however, I willingly grant that there is no couple in the habit of separating formally, and for ever.

I recollect a singular circumstance, in regard to this subject, which I witnessed in my own pigeon-house. Two of my young females having found no male, and finding themselves, consequently, consumed with the most ardent desire of pairing themselves, contracted for each other the most tender friendship; chose a nest in common; sought out for some clandestine intrigues; laid at length each their two eggs; hatched them in common, and brought up their young in the same manner as if they had been male and female.

XXI. *On the Direction of the Radicle and Germen during the Vegetation of Seeds. By* THOMAS ANDREW KNIGHT, *Esq. F.R.S. In a Letter to the Right Hon. Sir* JOSEPH BANKS, *K.B. P.R.S.**

MY DEAR SIR,

IT can scarcely have escaped the notice of the most inattentive observer of vegetation, that in whatever position a seed is placed to germinate, its radicle invariably makes an effort to descend towards the centre of the earth, whilst the elon-

* From the *Transactions of the Royal Society* for 1806.

gated

gated germen takes a precisely opposite direction; and it has been proved by Du Hamel * that if a seed, during its germination, be frequently inverted, the points both of the radicle and germen will return to the first direction. Some naturalists have supposed these opposite effects to be produced by gravitation; and it is not difficult to conceive that the same agent, by operating on bodies so differently organized as the radicle and germen of plants are, may occasion the one to descend and the other to ascend.

The hypothesis of these naturalists does not, however, appear to have been much strengthened by any facts they were able to adduce in support of it, nor much weakened by the arguments of their opponents; and therefore, as the phænomena observable during the conversion of a seed into a plant are amongst the most interesting that occur in vegetation, I commenced the experiments, an account of which I have now the honour to request you to lay before the Royal Society.

I conceived that if gravitation were the cause of the descent of the radicle, and of the ascent of the germen, it must act either by its immediate influence on the vegetable fibres and vessels during their formation, or on the motion and consequent distribution of the true sap afforded by the cotyledons: and as gravitation could produce these effects only whilst the seed remained at rest, and in the same position relative to the attraction of the earth, I imagined that its operation would become suspended by constant and rapid change of the position of the germinating seed, and that it might be counteracted by the agency of centrifugal force.

Having a strong rill of water passing through my garden, I constructed a small wheel similar to those used for grinding corn, adapting another wheel of a different construction, and formed of very slender pieces of wood, to the same axis. Round the circumference of the latter, which was eleven inches in diameter, numerous seeds of the garden bean, which had been soaked in water to produce their greatest degree of expansion, were bound, at short distances from

* Physique des Arbres.

each

each other. The radicles of these seeds were made to point in every direction; some towards the centre of the wheel, and others in the opposite direction; others as tangents to its curve, some pointing backwards, and others forwards, relative to its motion; and others pointing in opposite directions in lines parallel with the axis of the wheels. The whole was inclosed in a box, and secured by a lock, and a wire grate was placed to prevent the ingress of any body capable of impeding the motion of the wheels.

The water being then admitted, the wheels performed something more than 150 revolutions in a minute; and the position of the seeds relative to the earth was, of course, as often perfectly inverted, within the same period of time; by which I conceive that the influence of gravitation must have been wholly suspended.

In a few days the seeds began to germinate, and, as the truth of some of the opinions I had communicated to you, and of many others which I had long entertained, depended on the result of the experiment, I watched its progress with some anxiety, though not with much apprehension; and I had soon the pleasure to see that the radicles, in whatever direction they were protruded from the position of the seed, turned their points outwards from the circumference of the wheel, and in their subsequent growth receded nearly at right angles from its axis. The germens, on the contrary, took the opposite direction, and in a few days their points all met in the centre of the wheel. Three of these plants were suffered to remain on the wheel, and were secured to its spokes to prevent their being shaken off by its motion. The stems of these plants soon extended beyond the centre of the wheel; but the same cause which first occasioned them to approach its axis, still operating, their points returned and met again at its centre.

The motion of the wheel being in this experiment vertical, the radical and germen of every seed occupied, during a minute portion of time in each revolution, precisely the same position they would have assumed had the seeds vegetated at rest; and as gravitation and centrifugal force also acted in lines parallel with the vertical motion and surface

of

of the wheel, I conceived that some slight objections might be urged against the conclusions I felt inclined to draw. I therefore added to the machinery I have described another wheel, which moved horizontally over the vertical wheels; and to this, by means of multiplying wheels of different powers, I was enabled to give many different degrees of velocity. Round the circumference of the horizontal wheel, whose diameter was also eleven inches, seeds of the bean were bound as in the experiment which I have already described, and it was then made to perform 250 revolutions in a minute. By the rapid motion of the water-wheel much water was thrown upwards on the horizontal wheel, part of which supplied the seeds upon it with moisture, and the remainder was dispersed, in a light and constant shower, over the seeds in the vertical wheel, and on others placed to vegetate at rest in different parts of the box.

Every seed on the horizontal wheel, though moving with great rapidity, necessarily retained the same position relative to the attraction of the earth; and therefore the operation of gravitation could not be suspended, though it might be counteracted, in a very considerable degree, by centrifugal force; and the difference I had anticipated between the effects of rapid vertical and horizontal motion soon became sufficiently obvious. The radicles pointed downwards about ten degrees below, and the germens as many degrees above, the horizontal line of the wheel's motion; centrifugal force having made both to deviate 80 degrees from the perpendicular direction each would have taken, had it vegetated at rest. Gradually diminishing the rapidity of the motion of the horizontal wheel, the radicles descended more perpendicularly, and the germens grew more upright; and when it did not perform more than 80 revolutions in a minute, the radicle pointed out about 45 degrees below, and the germen as much above, the horizontal line, the one always receding from, and the other approaching to, the axis of the wheel.

I would not, however, be understood to assert, that the velocity of 250 or 80 horizontal revolutions in a minute will always give accurately the degrees of depression and elevation of the radicle and germen which I have mentioned; for the rapidity

dity of the motion of my wheels was sometimes diminished by the collection of fibres of conferva against the wire grate, which obstructed, in some degree, the passage of the water; and the machinery, having been the workmanship of myself and my gardener, cannot be supposed to have moved with all the regularity it might have done had it been made by a professional mechanic. But I conceive myself to have fully proved that the radicles of germinating seeds are made to descend, and their germens to ascend, by some external cause, and not by any power inherent in vegetable life; and I see little reason to doubt that gravitation is the principal if not the only agent employed, in this case, by nature. I shall therefore endeavour to point out the means by which I conceive the same agent may produce effects so diametrically opposite to each other.

The radicle of a germinating seed (as many naturalists have observed) is increased in length only by new parts successively added to its apex or point, and not at all by any general extension of parts already formed; and the new matter which is thus successively added, unquestionably descends in a fluid state from the cotyledons *. On this fluid, and on the vegetable fibres and vessels whilst soft and flexible, and whilst the matter which composes them is changing from a fluid to a solid state, gravitation, I conceive, would operate sufficiently to give an inclination downwards to the point of the radicle; and as the radicle has been proved to be obedient to centrifugal force, it can scarcely be contended that its direction would remain uninfluenced by gravitation.

I have stated that the radicle is increased in length only by parts successively added to its point: the germen, on the contrary, elongates by a general extension of its parts previously organized; and its vessels and fibres appear to extend themselves in proportion to the quantity of nutriment they receive. If the motion and consequent distribution of the true sap be influenced by gravitation, it follows, that when the germen at its first emission, or subsequently, deviates from a perpendicular direction, the sap must accumulate on its under side: and I have found in a great va-

* See Phil. Trans. of 1805.

riety

riety of experiments on the seeds of the horse-chestnut, the bean, and other plants, when vegetating at rest, that the vessels and fibres on the under side of the germen invariably elongate much more rapidly than those on its upper side; and thence it follows that the point of the germen must always turn upwards. And it has been proved that a similar increase of growth takes place on the external side of the germen when the sap is impelled there by centrifugal force, as it is attracted by gravitation to its under side when the seed germinates at rest.

This increased elongation of the fibres and vessels of the under side is not confined to the germens, nor even to the annual shoots of trees, but occurs and produces the most extensive effects in the subsequent growth of their trunks and branches. The immediate effect of gravitation is certainly to occasion the further depression of every branch, which extends horizontally from the trunk of the tree; and, when a young tree inclines to either side, to increase that inclination; but it at the same time attracts the sap to the under side, and thus occasions an increased longitudinal extension of the substance of the new wood on that side*. The depression of the lateral branch is thus prevented; and it is even enabled to raise itself above its natural level, when the branches above it are removed; and the young tree, by the same means, becomes more upright, in direct opposition to the immediate action of gravitation; nature, as usual, executing the most important operations by the most simple means.

I could adduce many more facts in support of the preceding deductions; but those I have stated, I conceive to be sufficiently conclusive. It has, however, been objected by Du Hamel (and the greatest deference is always due to his opinions), that gravitation could have little influence on the direction of the germen were it in the first instance protruded, or were it subsequently inverted, and made to point perpendicularly downwards. To enable myself to answer

* This effect does not appear to be produced in what are called weeping trees, the cause of which I have endeavoured to point out in a former memoir. Phil. Trans. 1804.

this

this objection I made many experiments on seeds of the horse-chestnut, and of the bean, in the box I have already described; and as the seeds there were suspended out of the earth, I could regularly watch the progress of every effort made by the radicle and germen to change their positions. The extremity of the radicle of the bean, when made to point perpendicularly upwards, generally formed a considerable curvature within three or four hours when the weather was warm. The germen was more sluggish; but it rarely or never failed to change its direction in the course of 24 hours; and all my efforts to make it grow downwards, by slightly changing its direction, were invariably abortive.

Another, and apparently a more weighty objection to the preceding hypothesis, (if applied to the subsequent growth and forms of trees,) arises from the facts that few of their branches rise perpendicularly upwards, and that their roots always spread horizontally; but this objection, I think, may be readily answered.

The luxuriant shoots of trees, which abound in sap, in whatever direction they are first protruded, almost uniformly turn upwards, and endeavour to acquire a perpendicular direction; and to this their points will immediately return if they are bent downwards during any period of their growth; their curvature upwards being occasioned by an increased extension of the fibres and vessels of their under sides, as in the elongated germens of seeds. The more feeble and slender shoots of the same trees will, on the contrary, grow in almost every direction, probably because, their fibres being more dry, and their vessels less amply supplied with sap, they are less affected by gravitation. Their points, however, generally show an inclination to turn upwards; but the operation of light, in this case, has been proved by Bonnet* to be very considerable.

The radicle tapers rapidly as it descends into the earth, and its lower part is much compressed by the greater solidity of the mould into which it penetrates. The true sap also continues to descend from the cotyledons and leaves, and

* Récherches sur l'Usage des Feuilles dans les Plantes.

occasions

occasions a continued increase of the growth of the upper parts of the radicle; and this growth is subsequently augmented by the effects of motion, when the germen has risen above the ground. The true sap is therefore necessarily obstructed in its descent; numerous lateral roots are generated, into which a portion of the descending sap enters. The substance of these roots, like that of the slender horizontal branches, is much less succulent than that of the radicle first emitted, and they are in consequence less obedient to gravitation; and therefore, meeting less resistance from the superficial soil than from that beneath it, they extend horizontally in every direction, growing with most rapidity, and producing the greatest number of ramifications, wherever they find most warmth, and a soil best adapted to nourish the tree. As these horizontal or lateral roots surround the base of the tree on every side, the true sap descending down its bark enters almost exclusively into them, and the first perpendicular root, having executed its office of securing moisture to the plant, whilst young, is thus deprived of proper nutriment, and, ceasing almost wholly to grow, becomes of no importance to the tree. The tap root of the oak, about which so much has been written, will possibly be adduced as an exception; but, having attentively examined at least 20,000 trees of this species, many of which had grown in some of the deepest and most favourable soils of England, and never having found a single tree possessing a tap root, I must be allowed to doubt that one ever existed.

As trees possess the power to turn the upper surfaces of their leaves and the points of their shoots to the light, and their tendrils in any direction to attach themselves to contiguous objects, it may be suspected that their lateral roots are by some means directed to any soil in their vicinity which is best calculated to nourish the plant to which they belong; and it is well known that much the greater part of the roots of an aquatic plant, which has grown in a dry soil, on the margin of a lake or river, have been found to point to the water; whilst those of another species of tree, which thrives best in a dry soil, have been ascertained to take an

opposite direction: but the result of some experiments I have made is not favourable to this hypothesis, and I am rather inclined to believe that the roots disperse themselves in every direction, and only become most numerous where they find most employment, and a soil best adapted to the species of plant. My experiments have not, however, been sufficiently varied, or numerous, to decide this question, which I propose to make the subject of future investigation,

I am, &c.

Elton, Nov. 22, 1805. T. A. KNIGHT.

XXII. *Chemico-Galvanic Observations communicated to the National Institute of Italy.* *By* M. L. BRUGNATELLI.

[Concluded from p. 66.]

§ VII.

Various Coatings, Incrustations, and other particular Compositions, are produced by galvanizing Metals in pure Water.

WIRES of very pure gold, plunged in distilled water and submitted to the Galvanism of the positive pole, in a separate tube, are covered in a few hours with a subtle coating and crust of a saffron yellow colour. This is not observed upon gold wires alloyed with copper, nor upon those of pure gold when negatively galvanized. But the same yellow coating is observed upon platina wires, whether the piles be of middling or more energetic force.

The nature of these incrustations has not been exactly ascertained. It seems that they ought to be attributed to a commencement of the solution of the metal by means of the oxymuriatic acid which is produced in the experiment.

The other metals present singular combinations. Some of them are oxidated; some combine with hydrogen; others unite with pure water by the Galvanic influence. The following are the details:

(a) *Hydro-*

(2) *Hydrogenated Hydruret of Gold obtained by Galvanism. Conversion of this Compound into pure Gold, on which depends the Weight as well as the Polarity of the Pieces of Gold observed by Ritter.*

It was often remarked, that the metal wires employed to galvanize pure water, formed blacker crusts on the negative side than on the other. The better to study these effects the author made use of well polished metal wires, which he plunged in a glass tube an inch high, and containing nearly an ounce of pure water; the wires communicating one with the negative pole of the pile and the other with the positive; they both descended, at the distance of three lines from each other, into the tube, which was closed at bottom.

Pure gold, being more easily oxidated by this process than other metals, may be more minutely examined. It was remarked that the wire on the negative side changed first; it was covered with a black substance, which increased by degrees, until the metal could be no longer recognised, and it was changed in a few hours into a spongy substance very much swelled. It sometimes presents itself under the appearance of beautiful arborizations, formed of small threads, which are implanted into one another. These intricate forms are not observed, except when the great piles are weakened. The water in which the wires are plunged remains pure, and does not appear to contain any foreign substance.

The author regards the black matter formed upon the wire as being a hydruret of hydrogenated gold; *i. e.* hydrogenated gold combined with water. This compound is inodorous, and almost insipid. It blackens linen when first thrown upon it, and it afterwards changes to a purple colour, which it also occasions applied to the skin.

Upon plunging into a vessel full of distilled water a single gold wire, attached to the negative pole on the one side, and on the other side a strip of moistened pasteboard communicating with the positive pole, this particular modification of gold is not obtained; a good deal of hydrogen gas is liberated on the side of the metallic wire, and the

 water

water becomes alkaline. The author supposed that this alkali might probably dissolve the hydrat of gold as it was formed, and thus hinder it from appearing upon this metal; but the substance not being soluble in pure soda, he abandoned the idea.

He made a gold wire descend from each of the poles of the pile into the same tube, which contained a solution of soda diluted in water. After a few hours Galvanic action the two wires were coated with a black crust, so slender that it could not be collected in a sufficient quantity for examination. The author thought that it was on one side as well as the other hydrogenated gold.

This compound loses its water and its hydrogen by the action of oxygen at its formation. The author proved it by the following curious experiment:

He took a gold wire covered with this substance, obtained in pure water galvanized with the two metals, as has been indicated, and he made this wire pass from the negative to the positive pole. He even adapted to the negative pole the gold wire which had belonged to the positive pole, always in the same water. The black substance was then seen to diminish in volume by little and little, as if contracting itself upon the wire, which by degrees resumed its colour and brilliancy, while the opposite wire was covered, in its turn, with hydruret of gold. This apparent metamorphosis was performed in a few minutes.

This hydruret of hydrogenated gold is a conductor of Galvanism; because the metallic wires, although completely covered with this substance, speedily decompose water as soon as they are put in communication with the pile.

The quickness of this union of hydrogen to gold by means of Galvanism, and the singularity of this production, made the author suppose that it was upon this new body, very different from pure gold, that the supposed polarity of pieces of gold observed by Ritter depends, and which Brugnatelli has shown not to exist, except in such of these pieces as were in communication with the negative pole. These pieces, when left for some time in the Galvanic circuit, and lodged

lodged in a fold of moist paper, become black, and colour the paper very sensibly by the formation of hydrogenated gold.

In order to explain what had taken place in this experiment, the author hydrogenated a little, by the process indicated, the extremity of a gold wire carefully cleaned. He afterwards tried this wire upon a frog, prepared after the manner of Galvani, by placing under the thighs of the animal the hydrogenated extremity, and resting the other, finely cleaned and polished, upon the moistened paper where the spine of the back rested. The frog became very strongly convulsed, and sometimes overturned the wire by its struggles. This effect was manifested upon the frog even when the gold wire had been hydrogenated, only in the weakest degree, by five or six minutes action of the pile. The author concluded from this, that the polarity observed by Ritter depended entirely upon the hydrogenation of the gold, an operation which renders it positive compared to gold which has not undergone it. Silver, copper, and other metals, particularly antimony, have equally evinced this curious phænomenon.

(b) *Hydruret of Silver and hydrogenated Silver obtained by Galvanism.*

The author had always remarked with surprise, how two wires of pure silver, submitted to the action of the two poles of the pile, in water, like those of gold, were both speedily converted into a gray and blackish substance. In order to procure a certain quantity of this substance for examination, he placed in the same vessel two large silver wires coming from the two poles of a strong pile, and three lines distant from each other. He left them twelve hours in this situation. On the negative side the liberation of gas was very sensible, but very little of it appeared round the other wire. At the expiration of the twelve hours he found a copious deposit in the recipient, and the wires were abundantly covered with a particular substance. That of the negative side was much more abundant, being of an obscure gray, swelled, and full of protuberances. It was collected in a fold of

 paper.

paper. That upon the positive side was also separated; it was black, less abundant, and adhering to the metallic wire, although one part had formed a black deposit above.

The gray deposit of the negative pole was dried in the air. The colour brightened in proportion as the drying advanced; and this powder, slightly rubbed with the burnishing stick, assumed the brilliant and white colour of the metal, and appeared to be nothing else than very pure silver. It was then a true combination of water and silver, as well as a hydrure of this metal,—a singular combination, and unknown until the present day; "because," adds the author, "the metallic hydrates, examined by Proust with so much exactitude, result from the combination of the water with the metal in the state of oxide, and not in that of regulus."

The crust and the black deposit coming from the positive pole were recognised to be hydrogenated silver. It blackened paper, linen, and the fingers; it was a little soluble in ammonia, and insoluble in the muriatic acid. It was not revived by the action of the solar rays, but very well by heat, on allowing the escape of hydrogen gas.

When the two silver wires in the two separate tubes were submitted to the Galvanic action, very little hydrogenated silver only appeared upon the negative wire in the form of black crust. There was a little upon the point in a flake.

(c) *Copper and Oxide of Copper oxygenated by the Galvanic Action.*

Two wires of very fine copper were galvanized for several hours, communicating with the two poles of the pile, and inserted in two separate tubes full of water. A black crust was seen upon the negative wire, which was easily separated from the metal which it covered, and which, when folded in paper, gave it a black tint; the copper from which it had been detached appeared of the utmost metallic brilliancy. When the two wires were galvanized in a single recipient, the matter decomposed upon the negative wire showed itself in the water of the vessel in the form of a small and very black arborization, in *alto relievo*, contrary from that upon the gold wire. Gas was liberated round the two wires. The

black

black substance of this second experiment was easily collected, and was discovered to be hydrogenated copper. It was insipid, and insoluble in water and ammonia; soluble in the nitric acid, which was not coloured blue, perhaps because the metal was not in sufficient quantity. The solution took place without effervescence.

The copper wire galvanized positively in water, in a separate tube, or in the same vessel where the negative galvanization takes place, is covered with oxide, which soon converts itself into hydrogenated green oxide, passing to the brown when dried by fire. The wire was coated with a crust of a steel colour, so slender that its nature could not be examined. When rubbed upon paper it left an ashy feel, and the copper under this crust was little brilliant, and coloured; a circumstance which did not take place upon the side of the negative pole.

Two copper wires, less pure than the preceding, and of the size of a writing-quill, were galvanized in two separate tubes. A whitish cloud was soon seen to descend from the positive wire, the matter of which even penetrated through a double piece of parchment with which the inferior opening of the tube had been closed; it arrived at the water of the recipient common to both tubes, and was there converted into hydrogenated oxide under the appearance of green flakes. Upon the negative side a very small quantity of black hydrogenated copper was perceived; an effect which the author attributes to the impurity of the copper. The experiment lasted twelve hours. The water of the two tubes, and even that of the recipient, became alkaline.

(d) *Oxygenated Muriate of Iron, Hydrogenated Oxide of Iron, and Alkaline Martial Tincture, produced by Galvanism.*

The author had observed, that by galvanizing in two separate tubes, full of pure water, two iron wires, the one positively and the other negatively, there was formed in the positive tube, in less than sixteen hours action, an oxygenated muriate of iron, *i. e.* with excess of oxide of iron; a circumstance which hindered the composition from reddening

the tincture of althea, and made it give with the prussiate of potash a white precipitate, which in a few minutes, and in the contact of the air, assumed a superb blue colour. We may conclude from this, that the iron in this salt is at the minimum of oxidation, and that, consequently, the first composition formed is simple muriate, and not oxygenated muriate, as Pacchiani supposed. There was no sensible quantity of crust upon the metallic wire, and scarcely did the surface of the negative wire assume the slightest tint of black; but it rendered the water into which it was plunged strongly alkaline.

This experiment was repeated with iron wires of a quarter of a line in diameter, submitted for twenty hours to the action of a strong pile. A certain quantity of oxide of iron was seen filtering through the two membranous folds which closed the tube at bottom, and a considerable quantity of a deposit of the colour of ochre appeared at the bottom of the water of the recipient common to both tubes: this precipitate had all the characters of a hydrogenated oxide of iron. A portion of this deposit, in contact with the parchment which formed the negative tube, was decomposed, and had passed to the deep black. The water of the common recipient was neither acid nor alkaline.

The author proceeds afterwards to the examination of the changes which happened to the two iron wires attached on one part to the poles of the pile, and plunging upon the other side in the water of a common recipient. Here, in the same manner, no sensible production of gas was observed on the positive side, while it was very abundant on the negative side. The water of the recipient soon became yellow; which proved that the iron was there in a particular state of combination.

Upon the positive side, besides the portion of muriate of iron which was dissolved in water, there was a very abundant oxide of this metal visibly precipitated, which was soon hydrogenated. It assumed a yellow colour and a flaky appearance; it was insipid, and insoluble in water. When collected upon white blotting-paper, this precipitate assumed a deep red colour. On exposure to a gentle heat, the colour changed

changed in proportion as the water evaporated, and nothing else remained than a blackish oxide.

The author concludes from these facts, that iron positively galvanized in water does not there experience its greatest possible degree of oxidation, and that the colour which it acquires happens from its particular combination with water; a combination hitherto unknown or neglected by chemists.

The negative wire was covered after several hours Galvanism with a portion of the oxide of hydrogenated iron which came from the positive pole. But this oxide was decomposed there, converted into alkaline oxide of iron of a yellow colour, and very soluble in water. It is this singular combination which occasions the yellow tint of the water of the recipient, a liquid thus converted into a true alkaline tincture of mars.

Towards the extremity of the wire a portion of the hydrogenated oxide of iron became blackened, and converted into hydrogenated iron of the deepest black.

The yellow water in this experiment did not form blue with the prussiate of potash, and scarcely greened the purple tincture of althea, which the author employed as the most sensible re-agent in the presence of the acids or the alkalis.

§ VIII.

Disengagement of the Carbonate of Soda in pure Water galvanized with Charcoal; Hydrogenated Charcoal; Project of a solid Vegetable Pile.

The author determined to examine what was the change which water underwent when galvanized by the intermedium of charcoal. He took out of a burning furnace some pieces of charcoal of about an inch and a half in length, and, after having extinguished and cooled them, he shaped them with a knife into slender strips of about three lines in size, and pierced at one extremity. An iron wire passed into this hole; and one of the wires leading from the charcoal thus prepared communicated with the positive pole, the other with the negative pole of a strong pile. The charcoal was more than half immersed in the water of a separate tube. The tubes were closed below with parchment,

ment, and they were all plunged into one vessel containing water.

The gas was abundantly liberated in the positive tube during the whole time of the experiment. At first only a few bubbles were seen upon the negative side, and they soon disappeared. After 24 hours of galvanization the author found carbonate of soda in the water of the negative pole. That of the positive tube, in which so much gas was disengaged, appeared to contain only a few atoms of this salt.

On the positive side the charcoal preserved its surface very black, but that of the negative charcoal was singularly whitened by combining with the hydrogen as it was forming.

It is the opinion of Kirwan (Phil. Trans. 1785.) and Berthollet (Stat. Chim. t. ii.) that charcoal contains hydrogen, because in certain circumstances it yields an inflammable gas: but the author strongly suspects that the inflammable gas obtained, in a close vessel, even from calcined charcoal, comes from a little humidity attracted from the atmosphere in cooling, and which is decomposed at the burning temperature; or perhaps that it is only the gaseous oxide of carbon, so well examined by Cruickshank. He founds this conjecture upon the observation which he had made, that charcoal scarcely beginning to be hydrogenated by Galvanism, changes all at once the electrometer character, as gold does; and from negative becomes positive, compared with other charcoal, and tried with the frog, after the method of Galvani. It is probable that upon thus giving the negative property to 100 disks of charcoal, and by uniting them to 100 other disks of pure carbon, we might form a solid and active vegetable pile, by placing, in the usual way, between the disks united two and two, rounds of wet pasteboard. Thus we might have a pile analogous to that of Ritter, which has been called the charging pile.

§ IX.

The Black Oxide of Manganese may be hydrogenated by Galvanism.—Character which distinguishes it.

The author remarked, in the course of all his experiments upon

upon metals and their oxides, that the former combined with hydrogen, and that the latter returned to the metallic state by the action of the negative side of the pile. He attributes this last effect to the more powerful affinity of the originating hydrogen. But the black oxide of manganese forms an exception. When it is galvanized in water negatively, no gas is disengaged; the oxide is not reduced; it does not become white, but it is united to hydrogen. One of the characters which distinguish it is the positive property which it acquires compared with oxide of manganese, which has not been submitted to this operation.

§ X.

Disengagement of an Alkali in distilled Water by the Contact of a single Metal, without the Electrometer Apparatus.

In the course of his experiments upon the galvanization of metals in pure water, and upon the influence of metals upon water, independently of the action of the pile of Volta, the author never remarked that water acquired acid properties by any contact, however long, with any given metal.

We know that zinc and iron decompose water at every temperature. The author placed fragments of each of these two metals in two separate phials with a volume of distilled water double their bulk; he left them in contact with this liquid until the decomposition had reduced it to two-thirds of its volume. The metals were sensibly oxidated; a great deal of hydrogen was disengaged; but the water underwent no sensible change.

He afterwards poured two ounces of distilled water upon five ounces of zinc, in small pieces, contained in a phial of eight ounces capacity, closing it with a ground stopper. He agitated the mixture strongly for a quarter of an hour. The water became muddy, and deposited a gray powder. It was agitated again for nearly five hours with little interruption; the quantity of gray powder was much augmented. The muddy water was decanted, and allowed to settle. It had a particular smell and a fetid taste. But what most surprised the author was, that it greened the spirituous tincture

of

of althea, and troubled a little the solutions of mercury and silver.

After washing the zinc employed in the above experiment in distilled water, the same process was repeated with it several times, and always with the same effects.

Filings of copper and of iron, treated in the same manner, also produced an alkaline substance in water. Mercury itself, after a long agitation in distilled water, also produced alkali in this liquid. We know that Priestley remarked that he had also formed a black oxide of mercury in water, and that the water acquired both smell and taste; but the alkaline matter which was formed or disengaged in his experiment had escaped him.

The pulverulent substances which are deposited in water in which metals have been agitated are oxides of the greatest tenuity. That of zinc is gray; that of copper brown; that of iron and mercury is black. And as the air contained in the phials in which these metals are agitated undergoes no sensible change, it appears evident that by this process the metal is united to the oxygen of the water, without any appreciable quantity of hydrogen gas being disengaged.

The water rendered alkaline by the processes which we have mentioned greens the purple tincture of althea; but if it is allowed to settle some hours upon these same metals, it loses its alkaline properties. It must follow, then, that the alkali is either decomposed, or that it enters into some new combination.

In order to determine the nature of this alkali, the author poured a little muriatic acid upon water alkalized by mercury and by zinc, and filtered. He afterwards evaporated it, and obtained a salt in short and transverse spicula; but it was in too small quantity to determine its nature with certainty. He ascertained, however, that it was not soda; and he had every reason to believe that it was muriate of ammonia.

General Reflections upon the preceding Observations.

"Some of the facts," says M. Brugnatelli, "communicated in this paper, and which are singular in themselves, may

may perhaps excite the curiosity of those chemists who follow Galvanic experiments. I have hitherto abstained from drawing any conclusion, because I saw the necessity of observing more and more attentively, and of collecting a greater number of facts, before attempting to establish any theory. Many researches still remain to be made, and many doubts to clear up.

"1. It must be ascertained whether the gas which is disengaged from charcoal in water positively galvanized, and from all those metals which in place of muriatic acid yield at length an alkali, is oxygen gas; or is it of another description?

"2. Whether the alkali which is manifested in water positively galvanized by the metals, is the same with that which is formed in water by the action of the negative pole, and which is soda.

"3. If the presence of water is essential to the formation of soda by Galvanism.

"4. What are the component parts of this alkali.

"5. If the carbonic acid with which the soda is saturated, that is produced in water negatively galvanized with charcoal, proceeds entirely from charcoal alone.

"6. If the Galvanic fluid, very active in its nature, and probably composed of something more than a subtle fluid, does not furnish some ingredients to the compounds which manifest themselves in the action of Galvanism.

"7. If we can obtain the same products upon galvanizing water by the above processes, without the contact of the atmospheric air, or in an atmosphere of gas which was foreign to it.

"8. If water is truly decomposed by the action of Galvanism, and by the intermedium of the metals, charcoal, and oxide of manganese.

"9. If the gases which are disengaged in water, the fixing of the oxygen and hydrogen in metals galvanized in this liquid, are uniformly and exclusively the effect of the decomposition.

"10. If the caloric which makes the gas elastic is produced by electricity, or from the water.

"11. If

"11. If all the chemical phænomena indicated and obtained from the Galvanic action may be also manifested by the influence of electrical currents produced by common machines; since the electric fluid does not appear to differ essentially from the Galvanic, although the contrary has been asserted.

"12. If the formation of an alkali by the contact of water with a single metal comes also from Galvanism, why is this alkali not soda? If it is that of ammonia, as there is reason to believe, how can this alkali be produced in the midst of a strong agitation and in distilled water, which contains no sensible quantity of azote?

"13. Why gold, platina, iron, and the black oxide of manganese, when positively galvanized in water, form muriatic acid, in contradiction to the other metals hitherto submitted to the same process.

"14. If the metals or the metallic oxides, which produce the muriatic acid by Galvanism, always liberate oxygen gas, (except iron alone, which is oxidated on this occasion,) why is muriatic acid not obtained with the other metals also which are oxidated in water, as well as iron, when positively galvanized in this liquid? Why can we not produce muriatic acid in water by means of iron and zinc, metals which take its oxygen from it even in the cold, and, without the assistance of any known electrometer apparatus, which are even oxidated themselves? and why is an alkali rather produced in this case?

"The Galvanic experiments upon which I am at present occupied, have for their object to resolve some of the questions I have now announced, and will form the subject of future publications."

XXIII. *Account of a Series of Experiments, showing the Effects of Compression in modifying the Action of Heat.* *By Sir* James Hall, *Bart. F.R.S. Lond. and Edin.*

[Continued from p. 26.]

§ VII.

Measurement of the Force required to constrain the Carbonic Acid.—Apparatus with the Muzzle of the Barrel upwards, and the Weight acting by a long Lever.—Apparatus with the Muzzle downwards.—Apparatus with Weight acting directly on the Barrel.—Comparison of various Results.

In order to determine, within certain limits at least, what force had been exerted in the foregoing experiments, and what was necessary to ensure their success, I made a number of experiments in a mode nearly allied to that followed by Count Rumford in measuring the explosive force of gunpowder.

I began to use the following simple apparatus in June 1803:—I took one of the barrels, made as above described, for the purpose of compression, having a bore of 0·75 of an inch *, and dressed its muzzle to a sharp edge. To this barrel was firmly screwed a collar of iron (*aa*, fig. 36) placed at a distance of about three inches from the muzzle, having two strong bars (*b b*) projecting at right angles to the barrel, and dressed square. The barrel, thus prepared, was introduced, with its breech downwards, into the vertical muffle (fig. 35); its length being so adjusted, that its breech should be placed in the strongest heat; the two projecting bars above described resting on two other bars (*cc*, fig. 35) laid upon the furnace to receive them; one upon each side of the muffle. Into the barrel so placed was introduced a cradle, containing carbonate, with all the arrangements formerly mentioned, the rod connected with it being of such length as just to lie within the muzzle of the barrel. The liquid metal was then poured in till it filled the barrel, and stood

* This was the size of barrel used in all the following experiments, where the fact is not otherwise expressed.

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at the muzzle with a convex surface; a cylinder of iron, of about an inch in diameter and half an inch thick, was laid on the muzzle (fig. 35 and 37), and to it a compressing weight was instantly applied. This was first done by the pressure of a bar of iron (*de*, fig. 35), three feet in length, introduced loosely into a hole (*d*), made for the purpose in the wall against which the furnace stood, the distance between this hole and the wall being one foot. A weight was then suspended at the extremity of the bar (*e*), and thus a compressing force was applied equal to three times that weight. In the course of practice, a cylinder of lead was substituted for that of iron, and a piece of leather was placed between it and the muzzle of the barrel, which last being dressed to a pretty sharp edge, made an impression in the lead: to assist this effect, one smart blow of a hammer was struck upon the bar, directly over the barrel, as soon as the weight had been hung on.

It was essential, in this mode of operation, that the whole of the metal should continue in a liquid state during the action of heat; but when I was satisfied as to its intensity and duration, I congealed the metal, either by extinguishing the furnace entirely, or by pouring water on the barrel. As soon as the heat began to act, drops of metal were seen to force themselves between the barrel and the leather, following each other with more or less rapidity, according to circumstances. In some experiments there was little exudation; but few of them were entirely free from it. To save the metal thus extruded, I placed a black lead crucible, having its bottom perforated, round the barrel, and luted close to it (fig. 37); some sand being laid in this crucible, the metal was collected on its surface. On some occasions, a sound of ebullition was heard during the action of heat; but this was a certain sign of failure.

The results of the most important of these experiments have been reduced to a common standard in the second table placed in the Appendix; to which reference is made by the following numbers:

No. 1.—On the 16th of June 1803, I made an experiment with these arrangements. I had tried to use a weight

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of 30 lbs. producing a pressure of 90 lbs.; but I found this not sufficient. I then hung on a weight of 1 cwt., or 112 lbs.; by which a compressing force was applied of 3 cwt., or 336 lbs. Very little metal was seen to escape, and no sound of ebullition was heard. The chalk in the body of the large tube was reduced to quicklime; but what lay in the inner tube was pretty firm, and effervesced to the last. One or two facettes, of good appearance, were likewise found. The contents of the small tube had lost but 2·6 per cent.; but there was a small visible intrusion of metal, and the result, by its appearance, indicated a greater loss. I considered this, however, as one point gained; that being the first tolerable compression accomplished by a determinate force. The pyrometer indicated 22°.

This experiment was repeated the same day, when a still smaller quantity of metal escaped at the muzzle; but the barrel had given way below, in the manner of those that have yielded for want of sufficient air. Even this result was satisfactory, by showing that a mechanical power, capable of forcing some of the barrels, could now be commanded. The carbonate in the little tube had lost 20 per cent.; but part of it was in a hard and firm state, effervescing to the last.

No. 2.—On the 21st of June I made an experiment with another barrel, with the same circumstances. I had left an empty space in the large tube, and had intended to introduce its muzzle downwards, meaning that space to answer as an air-tube; but it was inverted by mistake, and the tube, entering with its muzzle upwards, the empty space had of course filled with metal, and thus the experiment was made without any included air. There was no pyrometer used; but the heat was guessed to be about 25° where the subject of experiment lay. The barrel, when opened, was found full of metal, and the cradle being laid flat on the table, a considerable quantity of metal ran from it, which had undoubtedly been lodged in the vacuity of the large tube. When cold, I found that vacuity still empty, with a plating of metal. The tube was very clean to appearance, and, when shaken, its contents were heard to rattle. Above the

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little tube, and the cylinder of chalk, I had put some borax and sand, with a little pure borax in the middle, and chalk over it. The metal had not penetrated beyond the borax and sand, by a good fortune peculiar to this experiment; the intrusion of metal in this mode of execution being extremely troublesome. The button of chalk was found in a state of clean white carbonate, and pretty hard, but without transparency. The little tube was perfectly clean. Its weight, with its contents, seemed to have suffered no change from what it had been when first introduced. Attending, however, to the balance with scrupulous nicety, a small preponderance did appear on the side of the weight. This was done away by the addition of the hundredth of a grain to the scale in which the carbonate lay, and an addition of another hundredth produced in it a decided preponderance. Perhaps, had the tube, before its introduction, been examined with the same care, as great a difference might have been detected; and it seems as if there had been no loss, as least not more than one hundredth of a grain, which, on 10·95 grains, amounts to 0·0912, say 0·1 per cent. The carbonate was loose in the little tube, and fell out by shaking. It had a yellow colour, and compact appearance, with a stony hardness under the knife, and a stony fracture; but with very slight facettes, and little or no transparency. In some parts of the specimen, a whitish colour seemed to indicate partial calcination. On examining the fracture, I perceived, with the magnifier, a small globule of metal, not visible to the naked eye, quite insulated and single. Possibly the substance may have contained others of the same sort, which may have compensated for a small loss; but there could not be many such, from the general clean appearance of the whole. In the fracture, I saw here and there small round holes, seeming, though imperfectly, to indicate a beginning of ebullition.

I made a number of experiments in the same manner, that is to say, with the muzzle of the barrel upwards, in some of which I obtained very satisfactory results; but it was only by chance that the substance escaped the contamination of the fusible metal; which induced me to think

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of another mode of applying the compressing weight with the muzzle of the barrel downwards, by which I expected to repeat, with a determinate weight, all the experiments formerly made in barrels closed by congealed metal; and that, by making use of an air-tube, the air, rising to the breech, would secure the contents of the tube from any contamination. In this view, the barrel was introduced from below into the muffle with its breech upwards, and retained in that position by means of a hook fixed to the furnace, till the collar was made to press up against the grate, by an iron lever, loaded with a weight, and resting on a support placed in front. In some experiments made in this way, the result was obtained very clean, as had been expected; but the force had been too feeble, and when it was increased, the furnace yielded upwards by the mechanical strain.

I found it therefore necessary to use a frame of iron (as in fig. 38, the frame being represented separately in fig. 39), by which the brick-work was relieved from the mechanical strain. This frame consisted of two bars (*ab* and *fe*, figs. 38 and 39), fixed into the wall (at *a* and *f*), passing horizontally under the furnace, one on each side of the muffle, turning downwards at the front (in *b* and *e*), and meeting at the ground, with a flat bar (*cd*) uniting the whole. In this manner, a kind of stirrup (*bcde*) was formed in front of the furnace, upon the cross bar (*cd*) of which a block of wood (*hh*, fig. 38) was placed, supporting an edge of iron, upon which the lever rested, the working end of the lever (*g*) acting upwards. A strain was exerted, by means of the barrel and its collar, against the horizontal bars (*ab* and *fe*), which was effectually resisted by the wall (at *a* and *f*) at one end of these bars, and by the upright bars (*cb* and *de*) at the other end. In this manner the whole strain was sustained by the frame, and the furnace stood without injury.

The iron bar, at its working end, was formed into the shape of a cup (at *g*), and half filled with lead, the smooth surface of which was applied to the muzzle of the barrel. The lever, too, was lengthened, by joining to the bar of iron a beam of wood, making the whole ten feet in length.

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In this manner, a pressure upwards was applied to the barrel equal to the weight of 10 cwt.

In the former method, in which the barrel stood with its muzzle upwards, the weight was applied while the metal was liquid. In this case it was necessary to let it previously congeal, otherwise the contents would have run out in placing the barrel in the muffle, and to allow the liquefaction essential to these trials to be produced by the propagation of heat from the muffle downwards. This method required, therefore, in every case the use of an air-tube; for, without it, the heat acting upon the breech, while the metal at the muzzle was still cold, would infallibly have destroyed the barrel. A great number of these experiments failed, with very considerable waste of the fusible metal, which on these occasions was nearly all lost. But a few of them succeeded, and afforded very satisfactory results, which I shall now mention.

In November 1803, some good experiments were made in this way, all with a bore of 0·75, and a pressure of 10 cwt.

No. 3.—On the 19th a good limestone was obtained in an experiment made in a temperature of 21°, with a loss of only 1·1 per cent.

No. 4. On the 22d, in a similar experiment, there was little exudation by the muzzle. The pyrometer gave 31°. The carbonate was in a porous and almost frothy state.

No. 5.—In a second experiment, made the same day, the heat rose to 37° or 41°. The substance bore strong marks of fusion, the upper part having spread on the little tube: the whole was very much shrunk, and run against one side. The mass sparkling and white, and in a very good state.

No. 6.—On the 25th an experiment was made with chalk and some fragments of snail shell, with about half a grain of water. The heat had risen to near 51° or 49°. The barrel had been held tight by the beam, but was rent and a little swelled at the breech. The rent was wide, and such as has always appeared in the strongest barrels when they failed. The carbonate was quite calcined; it had boiled over the little tube, and was entirely in a frothy state, with large

and distinctly rounded air-holes. The fragments of shell, which had occupied the upper part of the little tube, had lost every trace of their original shape in the act of ebullition and fusion.

No. 7.—On the 26th a similar experiment was made, in which the barrel was thrown open, in spite of this powerful compressing force, with a report like that of a gun (as I was told, not having been present), and the bar was found in a state of strong vibration. The carbonate was calcined, and somewhat frothy; the heart of one piece of chalk used was in a state of saline marble.

It now occurred to me to work with a compressing force, and no air-tube, trusting, as happened accidentally in one case, that the expansion of the liquid would clear itself by gentle exudation, without injury to the carbonate. In this mode it was necessary, for reasons lately stated, to place the muzzle upwards. Various trials made thus, at this time, afforded no remarkable results. But I resumed the method, with the following alteration in the application of the weight, on the 27th of April 1804.

I conceived that some inconvenience might arise from the mode of employing the weight in the former experiments. In them it had been applied at the end of the bar, and its effect propagated along it, so as to press against the barrel at its other extremity. It occurred to me, that, the propagation of motion in this way requiring some sensible time, a considerable quantity of carbonic acid might escape, by a sudden eruption, before that propagation had taken effect. I therefore thought that more effectual work might be done by placing a heavy mass (fig. 40) so as to act directly and simply upon the muzzle of the barrel; this mass being guided and commanded by means of a powerful lever (*ab*). For this purpose I procured an iron roller, weighing 3 cwt. 7 lbs., and suspended it over the furnace to the end of a beam of wood, resting on a support near the furnace, with a long arm guided by a rope (*cc*) and pulley (*d*), by which the weight could be raised or let down at pleasure.

With this apparatus I made some tolerable experiments; but I found the weight too light to afford certain and steady

 results

results of the best quality. I therefore procured at the foundry a large mass of iron (f), intended, I believe, for driving piles, and which, after allowing for the counterpoise of the beam, gave a direct pressure of 8·1 cwt.; and I could, at pleasure, diminish the compressing force, by placing a bucket (e) at the extremity of the lever, into which I introduced weights, whose effect on the ultimate great mass was known by trial. Many barrels failed in these trials: at last I obtained one of small bore, inch 0·54, which gave two good results on the 22d of June 1804.

No. 8.—Wishing to ascertain the least compressing force by which the carbonate could be effectually constrained in melting heats, I first observed every thing standing firm in a heat of above 20°; I then gradually threw weights into the bucket, till the compressing force was reduced to 2 cwt. Till then, things continued steady; but, on the pressure being still further diminished, metal began to ooze out at the muzzle with increasing rapidity. When the pressure was reduced to 1½ cwt. air rushed out with a hissing noise. I then stopped the experiment, by pouring water on the barrel. The piece of chalk had lost 12 per cent. It was white and soft on the outside, but firm and good in the heart.

No. 9.—An experiment was made with chalk, in a little tube; to this, one grain of water was added. I had intended to work with 4 cwt. only; but the barrel was no sooner placed, than an exudation of metal began at the muzzle, owing, doubtless, to the elasticity of the water. I immediately increased the pressure to 8·1 cwt. by removing the weight from the bucket; when the exudation instantly ceased. I continued the fire for three quarters of an hour, during which time no exudation happened; then all came out remarkably clean, with scarcely any contamination of metal. The loss amounted to 2·58 per cent. The substance was tolerably indurated, but had not acquired the character of a complete stone.

In these two last experiments, the bore being small, a pyrometer could not be admitted.

On the 5th of July 1804, I made three very satisfactory experiments

experiments of this kind, in a barrel with the large bore of 0·75 of an inch.

No. 10. was made with a compressing force of only 3 cwt. A small eruption at the muzzle being observed, water was thrown on the barrel: the pyrometer gave 21°: the chalk was in a firm state of limestone.

No. 11. with 4 cwt. The barrel stood, without any eruption or exudation, till the heat rose to 25°. There was a loss of 3·6 per cent.: the result was superior, in hardness and transparency, to the last, having somewhat of a saline fracture.

No. 12. with 5 cwt. The result, with a loss of 2·4 per cent., was of a quality superior to any of those lately obtained.

These experiments appear to answer the end proposed, of ascertaining the least pressure, and lowest heat, in which limestone can be formed. The results, with various barrels of different sizes, agree tolerably, and tend to confirm each other. The table shows, when we compare Nos. 1, 2, 8, 10, 11, 12, that a pressure of 52 atmospheres, or 1700 feet of sea, is capable of forming a limestone in a proper heat: that under 86 atmospheres, answering nearly to 3000 feet, or about half a mile, a complete marble may be formed: and lastly, that with a pressure of 173 atmospheres, or 5700 feet, that is, little more than one mile of sea, the carbonate of lime is made to undergo complete fusion, and to act powerfully on other earths.

§ VIII.

Formation of Coal.—Accidental Occurrence which led me to undertake these Experiments.—Results extracted from a former Publication.—Explanation of some Difficulties that have been suggested.—The Fibres of Wood in some Cases obliterated, and in some preserved, under Compression.—Resemblance which these Results bear to a Series of Natural Substances described by Mr. Hatchett.—These Results seem to throw Light on the History of Surturbrand.

As I intend, on some future occasion, to resume my experiments with inflammable substances, which I look upon

as far from complete, I shall add but a few observations to what I have already laid before this society, in the sketch I had the honour to read in this place on the 30th of August last.

The following incidental occurrence led me to enter upon this subject rather prematurely, since I had determined first to satisfy myself with regard to the carbonate of lime.

Observing, in many of the last-mentioned class of experiments, that the elastic matters made their escape between the muzzle of the barrel and the cylinder of lead, I was in the habit, as mentioned above, of placing a piece of leather between the lead and the barrel; in which position, the heat to which the leather was exposed was necessarily below that of melting lead. In an experiment, made on the 28th November 1803, in order to ascertain the power of the machinery, and the quantity of metal driven out by the expansion of the liquid, there being nothing in the barrel but metal, I observd, as soon as the compressing apparatus was removed, (which on this occasion was done while the lower part of the barrel was at its full heat, and the barrel standing brim full of liquid metal,) that all the leather which lay on the outside of the circular muzzle of the barrel remained, being only a little browned and crumpled by the heat to which it had been exposed. What leather lay within the circle, had disappeared; and, on the surface of the liquid metal, which stood up to the lip of the barrel, I saw large drops of a shining black liquid, which, on cooling, fixed into a crisp black substance, with a shining fracture, exactly like pitch or pure coal. It burned, though not with flame. While hot, it smelt decidedly of volatile alkali. The important circumstance here, is the different manner in which the heat has acted on the leather, without and within the rim of the barrel. The only difference consisted in compression, to which, therefore, the difference of effect must be ascribed: by its force, the volatile matter of the leather which escaped from the outward parts, had within the rim been constrained to remain united to the rest of the composition, upon which it had acted as a flux, and the whole together had entered into a liquid state, in a very low heat.

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Had the pressure been continued till all was cool, these substances must have been retained, producing a real coal.

On the 24th of April 1803, a piece of leather used in a similar manner (the compressing force being continued, however, till all was cold) was changed to a substance like glue, owing doubtless to compression, in a heat under that of melting lead.

These observations led me to make a series of experiments with animal and vegetable substances, and with coal; the result of which I have already laid before the society. I shall now repeat that communication, as printed in Nicholson's Journal for October last (1804).

" I have likewise made some experiments with coal, treated in the same manner as the carbonate of lime: but I have found it much less tractable; for the bitumen, when heat is applied to it, tends to escape by its simple elasticity, whereas the carbonic acid in marble is in part retained by the chemical force of quicklime. I succeeded, however, in constraining the bituminous matter of the coal, to a certain degree, in red heats, so as to bring the substance into a complete fusion, and to retain its faculty of burning with flame. But I could not accomplish this in heats capable of agglutinating the carbonate; for I have found, where I rammed them successively into the same tube, and where the vessel has withstood the expansive force, that the carbonate has been agglutinated into a good limestone, but that the coal has lost about half its weight, together with its power of giving flame when burnt, remaining in a very compact state, with a shining fracture. Although this experiment has not afforded the desired result, it answers another purpose admirably well. It is known, that where a bed of coal is crossed by a dike of whinstone, the coal is found in a peculiar state in the immediate neighbourhood of the whin; the substance in such places being incapable of giving flame, it is distinguished by the name of *blind coal*. Dr. Hutton has explained this fact, by supposing that the bituminous matter of the coal has been driven by the local heat of whin into places of less intensity, where it would probably be retained by distillation. Yet the whole must have been carried

ried on under the action of a pressure capable of constraining the carbonic acid of the calcareous spar, which occurs frequently in such rocks. In the last-mentioned experiment we have a perfect representation of the natural fact; since the coal has lost its petroleum, while the chalk in contact with it has retained its carbonic acid.

" I have made some experiments of the same kind, with vegetable and animal substances. I found their volatility much greater than that of coal, and I was compelled, with them, to work in heats below redness; for, even in the lowest red heat, they were apt to destroy the apparatus. The animal substance I commonly used was horn, and the vegetable, saw-dust of fir. The horn was incomparably the most fusible and volatile of the two. In a very slight heat it was converted into a yellow red substance, like oil, which penetrated the clay tubes through and through. In these experiments I therefore made use of tubes of glass. It was only after a considerable portion of the substance had been separated from the mass that the remainder assumed the clear black peculiar to coal. In this way I obtained coal both from saw-dust and from horn, which yielded a bright flame in burning.

" The mixture of the two produced a substance having exactly the smell of soot or coal tar. I am therefore strongly inclined to believe, that animal substance, as well as vegetable, has contributed towards the formation of our bituminous strata. This seems to confirm an opinion, advanced by Mr. Keir, which has been mentioned to me since I made this experiment. I conceive that the coal which now remains in the world is but a small portion of the organic matter originally deposited: the most volatile parts have been driven off by the action of heat before the temperature had risen high enough to bring the surrounding substance into fusion, so as to confine the elastic fluids, and subject them to compression.

" In several of these experiments I found, when the pressure was not great, when equal, for instance, only to eighty atmospheres, that the horn employed was dissipated entirely, the glass tube which had contained it being left almost

almost clean; yet undoubtedly, if exposed to heat without compression, and protected from the contact of the atmosphere, the horn would leave a cinder or coke behind it, of matter wholly devoid of volatility. Here, then, it would seem as if the moderate pressure, by keeping the elements of the substance together, had promoted the general volatility, without being strong enough to resist that expansive force, and thus, that the whole had escaped. This result, which I should certainly not have foreseen in theory, may, perhaps, account for the absence of coal in situations where its presence might be expected on principles of general analogy."

Since this publication, a very natural question has been put to me. When the inflammable substance has lost weight, or when the whole has been dissipated in these experiments, what has become of the matter thus driven off?

I must own, that to answer this question with perfect confidence, more experiments are required. But, in the course of practice, two circumstances have occurred as likely, in most cases, to have occasioned the loss alluded to. I found in these experiments, particularly with horn, that the chalk, both in powder and in lump, which was used to fill vacuities in the tubes, and to fix them in the cradle, was strongly impregnated with an oily or bituminous matter, giving to the substance the qualities of a stinkstone. I conceive that the most volatile part of the horn has been conveyed to the chalk, partly in a state of vapour, and partly by boiling over the lips of the glass tube, the whole having been evidently in a state of very thin fluidity. Having, in some cases, found the tube, which had been introduced full of horn, entirely empty after the experiment, I was induced, as above stated, to conceive, that, under pressure, it had acquired a greater general volatility than it had in freedom; and I find that, in the open fire, horn yields a charcoal equal to 20 per cent. of the original weight. But more experiments must be made on this subject.

Another cause of the loss of weight lay undoubtedly in the excess of heat employed in most of them to remove the cradle from the barrel. With inflammable substances no air-

air-tube was used, and, the heats being low, the air lodged in interstices had been sufficient to secure the barrels from destruction by the expansion of the liquid metal. In this view, likewise, I often used lead, whose expansion in such low heats I expected to be less than that of the fusible metal. And the lead requiring to melt it a heat very near to that of redness, the subject of experiment was thus, on removing the cradle, exposed in freedom to a temperature which was comparatively high. But, observing that a great loss was thus occasioned, I returned to the use of the fusible metal, together with my former method of melting it, by plunging the barrel, when removed from the furnace, into a solution of muriate of lime, by which it could only receive a heat of 250° of Fahrenheit.

The effect was remarkable in the few experiments tried in this way. The horn did not, as in the other experiments, change to a hard black substance, but acquired a semi-fluid and viscid consistency, with a yellow red colour, and a very offensive smell. This shows that the substances which here occasioned both the colour and smell of the results, had been driven off in the other experiments by the too great heat applied to the substance when free from compression.

I found that the organization of animal substance was entirely obliterated by a slight action of heat, but that a stronger heat was required to perform the entire fusion of vegetable matter. This, however, was accomplished; and in several experiments pieces of wood were changed to a jet black and inflammable substance, generally very porous, in which no trace could be discovered of the original organization. In others, the vegetable fibres were still visible, and are forced asunder by large and shining air-bubbles.

Since the publication of the sketch of my experiments I have had the pleasure to read Mr. Hatchett's very interesting account of various natural substances nearly allied to coal, and I could not help being struck with the resemblance which my results bear to them, through all their varieties, as brought into view by that able chemist; that resemblance affording a presumption, that the changes which, with true scientific modesty, he ascribes to an unknown cause, may

have

have resulted from various heats acting under pressure of various force. The substance to which he has given the name of *retinasphaltum* seems to agree very nearly with what I have obtained from animal substance, when the barrel was opened by means of low heat. And the specimen of wood entering into fusion, but still retaining the form of its fibres, seems very similar to the intermediate substance of Bovey coal and *surturbrand*, which Mr. Hatchett has assimilated to each other. It is well known that the surturbrand of Iceland consists of the stems of large trees, flattened to thin plates by some operation of nature hitherto unexplained. But the last-mentioned experiment seems to afford a plausible solution of this puzzling phænomenon.

In all parts of the globe we find proofs of slips, and various relative motions, having taken place amongst great masses of rock, whilst they were soft in a certain degree, and which have left unequivocal traces behind them, both in the derangement of the beds of strata, and in a smooth and shining surface, called *slickenside*, produced by the direct friction of one mass on another. During the action of subterranean heat, were a single stratum to occur, containing trees intermixed with animal substances, shell fish, &c., these trees would be reduced to a soft and unctuous state, similar to that of the piece of wood in the last-mentioned experiment, whilst the substance of the contiguous strata retained a considerable degree of firmness. In this state of things, the stratum just mentioned would very naturally become the scene of a slip, occasioned by the unequal pressure of the surrounding masses. By such a sliding motion, accompanied by great compression, a tree would be flattened, as any substance is ground in a mortar, by the combination of a lateral and direct force. At the same time the shells along with the trees would be flattened like those described by Bergman, while those of the same species in the neighbouring limestone rock, being protected by its inferior fusibility, would retain their natural shape.

[To be continued.]

XXIV. *Re-*

XXIV. *Report of Surgical Cases in the City Dispensary from the 1st of January to the 30th of June 1806. Communicated by* JOHN TAUNTON, *Esq. Surgeon to the City and Finsbury Dispensaries, and Lecturer on Anatomy, Surgery, &c.*

SINCE the last report (Philosophical Magazine, vol. xxiii. p. 312.) there have been admitted into this dispensary 362 patients.

Cured - -	215
Relieved - -	17
Irregular - -	5
Dead - -	2
Under cure - -	123
	362

Sixty-six have been visited at home, and nine have undergone operations.

In the surgical report (see January 1806) there were 115 patients under cure, of whom 107 have been cured, 6 relieved, and 2 are not known.

Surgical Cases.

S. B., æt. 26, observed a tumour in her left breast for several years, without being enabled to assign any cause for its appearance, having a hard irregular surface, attended with darting pain, and occasionally with a sensation of heat. The emplastrum hydrarg. cum ammoniac. was applied, the camphorated and saponaceous liniments were used, and other gentle stimulant applications, from which, in many cases, the most happy results have been experienced. This mode of treatment having been persisted in for several months without any benefit being obtained, (the tumour had even become larger, and was attended with more pain,) as there was no doubt with respect to the nature of the case, it was thought right to propose the extirpation of it by excision; to which the patient most readily acceded, and the operation was performed on the 16th of April. As the disease

ease was circumscribed, and situated at the upper part of the breast, the removal of the whole gland was not necessary; and the integuments being free from disease, a single incision only was required to expose the tumour; on the removal of which, it was found to have taken on the true scirrhous structure throughout its whole substance.

She underwent the operation with the greatest fortitude, and only complained of a little smarting pain for about one hour after. She slept well at night, and appeared perfectly easy in every respect on the following day, when two table spoonfuls of the subsequent mixture were ordered to be taken twice in the day.

℞ Aq. am. acet.
Aq. menth. āā ℥ iii. M.

On the third day, ℞ magn. vit. ℥ ii. Inf. Quas. ℥ viii. M. Cochl. iii. ampl. pro re nata.

These were the only medicines required. She sat up on the third day without the least inconvenience: on the fifth day the wound was dressed, which had nearly healed, and she recovered from one of the most formidable operations without experiencing one unfavourable symptom.

Eliz. J., æt. 20, naturally of a strong healthy constitution, has had symptoms of stone in the bladder for about three years, during which time she has been under the care of several professional gentlemen, none of whom appear to have had any idea of her complaint; and the gentleman under whose care she was placed for several weeks during the last autumn, observed to her mistress, "that some young fellow had been playing the rogue with her:" intimating that it was something connected with the lues venerea.

On the 8th of January she was admitted into the City Dispensary, the symptoms strongly characterizing the disease. She was sounded in a few days, and a stone discovered. The operation was performed on the 26th of February. The stone resembled a flattened sphere, was of a firm texture, and weighed near three ounces.

She took a mixture of aq. am. acet. cum aq. menth. āā, for the two first days; and on the third day some magn. vit.

vit. cum jalap. These were all the medicines that were necessary, as she had neither fever, nor any unfavourable symptoms, and continued to recover fast, being up on the fourth day; and on the sixth day was enabled to retain her water, when still; and in a short time her health was completely reinstated.

Jane R., æt. 35, of a delicate constitution and low stature, has had a tumour in the left breast for several years, hard and irregular on its surface, and frequently attended with hot darting pain. Several eminent surgeons have been consulted, all of whom concurred in its being of a true scirrhous nature, and that it should be removed by excision.

She was admitted a patient in the City Dispensary on the 19th of February, and the operation was performed on the 25th of the same month. The breast was naturally small, but from the extent of the disease it was necessary to remove the whole gland: the integuments were brought together and supported by one suture, and straps of adhesive plaister.

The smarting pain from the operation soon left her; but as there was some heat on the surface of the body, and the pulse rather quick, the following draught was ordered to be taken in the evening:

℞ Tinct. opii gtt. xl.
Aq. am. acet. ℥ ss. M. F. haust. o. n. sumend.

26th. A gentle perspiration was diffused over the whole body; and, having slept all night, she was perfectly easy, and expressed herself desirous of having what might be thought proper to eat, as she missed her dinner of the preceding day.

She sat up on the third day after the operation, and in a few days her health was much improved, not having occasion to take any medicines, except a few doses of magn. vit. cum jalap. On removing the dressing on the seventh day from the operation, the wound had completely united by the first intention, and remained perfectly easy till the 13th day of March; when she complained of much smarting pain; and on the removal of the bandages the integuments were inflamed to some extent towards the axilla, which was occasioned by exposure to cold the preceding day. The ceratum

tum saponis was the only application necessary, as the inflammation soon dispersed, and her health and strength were soon reinstated, equal to what she had enjoyed at any period prior to the present disease, from which she has suffered much both in body and mind.

Greville-street, Hatton-garden,
July 21, 1806.

XXV. *Thirtieth Communication from* Dr. Thornton, *relative to Pneumatic Medicine.*

A Case of Consumption cured by the Inhalation of Hydrogen Gas.

To Mr. Tilloch.

No. 1, Hinde-street, Manchester-square, July 10, 1806.

DEAR SIR,

Mr. Price, No. 20, Clarges-street, had a violent cough, expectoration, and all the symptoms of consumption.

Friday, October 20.

Order.

No. 1. { ℞ Kali sulphurat. dr. 2.
F. Pulv. }

Direction.

1. Take the powder No. 1. and dissolve it in a cup of milk. Drink half of it going to bed, and the remainder early the next morning, every day.

2. Inhale a quart of hydrogen air to ten of atmospheric, every day.

No. 2. ℞ Syr. pap. alb. unc. 6.

3. Take every night two table-spoonsful of No. 2.

Monday, October 23.

1. Continue on the air as ordered last.
2. Continue on No. 2. as ordered last.

Friday, October 27.

1. Continue on the air every other day.

No. 3.	℞ Flor. sulph. lot.	
	Carbon (charcoal) āā unc. ½	
	Syr. simp. q. s.	
	F. Elect.	

2. Take the size of a nutmeg of No. 3. every night and morning.

3. Take No. 2. as last ordered, every night.

Monday, October 30.

No. 4.	℞ Decoct. cinchon. - -	unc. 1½
	Tinct. cinchon. comp. -	dr. 2
	Myrrh. pulv. - - -	gr. 12
	F. Haustus.	

1. Continue the directions last ordered.
2. Take in addition No. 4. every evening at six o'clock.

Wednesday, November 8.

No. 5.	℞ Calomel - - -	gr. 2
	Rhei pulv. - - -	gr. 10
	F. Pil. 6.	

1. Take the six pills from box No. 5. to-night, going to bed.

No. 6.	℞ Rhei pulv. - - -	gr. 10
	Kali vitriolat. - -	scr. 1
	Syr. simp. - - -	dr. 2
	Aq. cinnam.	
	Aq. menth. pip. āā	dr. 7
	F. Haustus.	

2. Take No. 6. to-morrow morning, rising.

Friday, November 10.

Repeat the directions given October 30.

November 14.

Appeared in health, and was dismissed cured.

Observations on this Case.

1. At first the system was dis-oxygenated by sulphur and charcoal, and opium.

2. Then bark and myrrh were cautiously used.

3. The

3. The bowels were then relieved; and by taking off at the same time local inflammation of the lungs, by a reduced air being inhaled, and by medicines internally, being a cautious application of bark, a speedy cure was accomplished.

4. *Query:* Would any other means have saved this person's life?

I have the honour to remain, dear sir,
Your obliged devoted servant,
ROBERT JOHN THORNTON.

XXVI. *Description of an improved Geometrical Plotting Quadrant, Level, and Calculator, for the Use of Navigation and Land Surveying, ascertaining inaccessible Distances; and for demonstrating and determining various Problems in Geometry and Trigonometry. By Mr.* ROBERT SALMON, *of Woburn**.

ON the instrument and parts thereof are engraved the names given by the inventor, and made use of in these explanations; the base line being that at right angles with the 90 degrees on the arch, as it is also to the perpendicular, which perpendicular always moves parallel to the 90 degrees. For the use of land surveying, where the instrument can be made stationary, the sight (marked *a*, fig. 1. Pl. IV.) with the small hole in it must be applied; but for sea service, the one *b*, fig. 2. with the mirror, must be substituted in its place.

Every person who has had occasion to describe or calculate the parts of the right-lined figures used in geometry, perspective, surveying, navigation, dialing, architecture, &c. &c. must have perceived that all of them are resolvable into the most simple of figures—a triangle, or some number of them.

Hence the great importance of geometry and trigonome-

* From *Transactions of the Society of Arts*, &c. 1806.—The silver medal of the society, and ten guineas, were voted to Mr. Salmon for this communication. One of the instruments is preserved in the society's repository, for the inspection of the public.

 try,

try, in teaching, either by construction or calculation, the knowledge of all the properties or relations between the three sides and three angles, of which every plane triangle is composed. Euclid having demonstrated, in the fourth proposition of the sixth book of his Elements, that in any two similar triangles (by which he means their having the same angles, without regard to the actual lengths of their sides, for one triangle may be very small and the other ever so large,) every pair of the corresponding sides in the two triangles are proportional; it is the business of trigonometry to solve such problems, with the help of the tables of sines and tangents, or of sectors, sliding or other rules, and scales, by which you can find, on inspection, a right-angled triangle, exactly similar to any given right-angled triangle (or having one of its angles equal to 90°) which can be proposed, or can occur in practice; and by the Rule of Three we say, As any side of the tabular triangle is to the similar side, supposed to be known, of the triangle under consideration, so is any other side of the same tabular triangle to the corresponding side supposed to be sought of the triangle in question. It is evident, that by means of the base line, perpendicular, and either the upper or lower limb of my instrument, by the two motions of which the perpendicular is capable, and the angular motion of which the limbs are capable, any right-angled triangle whatsoever, as CBE or CDE, in the diagram fig. 6. may be instantly formed (by bringing the top corner of the perpendicular to touch the limb) with the same or greater facility than it could be taken out of a trigonometrical table, measured by the compasses on the sector, or set on any instrument now in use for that purpose. But no instrument that I have seen or read of is capable of forming immediately any obtuse-angled triangle, as on my geometrical plotting quadrant can be done; nor can the trigonometrical tables be applied to produce the sides and angles of such a triangle without some trouble in any case, and in some of the most useful cases in practice the labour is very considerable. I shall therefore give the solution of five problems. First, supposing that fig. 6. represents my instrument, set to answer this and the

following problems; A, B, C, being the triangle under consideration; then, since the ∠ ACE is by Euclid (I. 20.) equal to the ∠ BAC, it is evident that this angle will be shown, or may be set, by means of the divisions on the arc FG: also, that since CBE and ICB are also equal, the arc HI, with the addition of 90° (for the angle EBA), will show the ∠ CBA of the triangle; it is equally evident that the arc FH will show the sum of the two angles BCA and ACF, at the same time that the lengths of all the sides may be read off, on the divisions or scales, on CA, CB, and BA. Therefore,

First.—*To construct or set a Triangle, having two of its Angles and the Side between them given.*

Set the limb CG, to the division at G, upon the arc answering to one of the angles, say A, and make it fast; then to this ∠ A add the other given angle (which we will call C), and set the other limb CH, and make it fast at the division H, on the arc answering to the sum of their degrees; then on the limb CG seek the length of the given side CA; next, push the perpendicular up or down, till the parallel cuts the point A, (always observing the divided edges are those you work to) and by the help of the mill-headed nut move the perpendicular till its top corner just touches the limb CH, say in the point B, when it is evident that the degrees on the arc HI, added to 90°, is equal to the angle B, and that the other sides, CB and BA, may be read off thereon. Or supposing CBD to be the triangle, whose angles B and C, and side BC, are given, we have only to move the limbs so as to make IH equal to B, and HG equal to C, and then to bring the top of the perpendicular to touch CH, at the division B, answering to the side CB, when the other angle D will be shown by the division on the arc GF, adding 90° thereto; and the remaining sides CD and BD may be read off on their respective scales.

Second.—*To set a Triangle, having two Sides and the Angle included between them given.*

Let ABC be the triangle, AB and AC the given sides, and A the given angle; first set the limb CG to the division

answering to A, then bring the parallel up to the point A answering to the side CA, and by the nut move the perpendicular till BA answers to the given side BA; next bring down the limb CH to touch B, and on CB may be read the other side, while HG will show the angle C, and IH + 90° the ∠ B, whence all the six parts are known.

Third.—*To set a Triangle having two Sides, and an Angle opposite to one of them given.*

Let ABC be the triangle, AC and CB the given sides, and A the given angle: first read the angle A on FG, and set the limb CG thereto; then push up the parallel to the division at A, answering to CA, and with one hand work the nut and with the other move the limb CH, till they touch at B, the division answering to the side CB; then BA is the side sought, and the arc GH will show the ∠ C, and IH + 90° the ∠ B.

Fourth.—*To set a Triangle having two Angles, and a Side opposite to one of them given.*

Let ABC be the triangle, A and C the given angles, and BA the given side: first, set FG to the angle A, and GH to the angle C; then push the perpendicular up or down with one hand, while the other works the nut, till the given side BA, on the parallel, is applied exactly between the limbs CH and CG; then IH + 90° will show the remaining angle B; and on CB and CA may be read the lengths of those sides.

Fifth.—*To set a Triangle whose three Sides are given.*

Let ABC be the triangle; on the limb CH seek the point B, answering to the side CB; then, using one hand to move the perpendicular, and the other to turn the nut, let an assistant at the same time, with his right hand, gently move the limb CH, while you cause the top corner of the perpendicular always to touch the point B; at the same time let the assistant move the limb CG with his left hand, till the lengths of CA and BA on their respective scales are found to intersect each other, when FG will show the ∠ A, GH the ∠ C, and HI + 90° the ∠ B.

My

My solution to the last problem is inferior to the common method of plotting the triangle on paper, and measuring the angles with a protractor; but I have introduced it here to show that my instrument is capable of solving this as well as all other cases of obtuse-angled triangles, and might, by extending the arc to a semicircle, as shown by the dotted lines on the figure, solve any triangle. In the practical problems in surveying, which follow, the triangles can always be taken right- or obtuse-angled, and the instrument as at present constructed is fully competent. I might here add, that a given line can readily, by my instrument, be divided into any number of equal parts; drawings might be enlarged or diminished as readily as with the proportional compasses, and many other equally useful purposes may be effected thereby.

First.—*To measure an inaccessible Distance by a perpendicular Line set off towards the right Hand, from the Line or Base between the Observer and Object.*

Set the base line of the instrument in a line pointing to the object, at the same time place a staff at any distance, at pleasure, as a perpendicular (being 90 degrees from the base). On this perpendicular measure any distance (say 50 yards or other measures) as a second station; move the instrument to this distance, and place it with its perpendicular in the same line as before; the instrument being so placed, set the lower limb pointing to the object, and with the screw make the same fast; this done, the distance of the object will be thus readily known. Raise the moving perpendicular of the instrument to the divsion 50 (as before suggested), then with this height move the same, by means of the nut, till the extremity intersects exactly the lower limb before set, at which intersection the distance from the second station will be shown; and on the base line will also at the same time be seen the distance from the first station: this is a case of right-angled triangles.

Note. As the divisions on the perpendicular are denominated (either feet, yards, poles, or other measures), so will the distances be indicated on the other limbs, and on the base of the instrument.

 Secondly.

Secondly.—*To determine the Distances of any two inaccessible Objects, both Objects lying in a right Line from the Observer.*

As before directed, place the instrument with its base in the line of the objects; then by means of the upper limb, set at 90 degrees, place a staff as a perpendicular at any distance at pleasure (say 50, as before). This done, remove the instrument to this second station, and place it so that the upper limb (still at 90°) may be in the same line as when at the first station: this done, move the upper limb into the direction of the nearest, and the lower limb into the direction of the most distant object; which limbs being so set, and made fast, the distance of both objects from the second station will be seen on the two limbs, and the distance from the first station at the same time seen on the base line, by setting and moving the perpendicular as directed in the last case. This is also a case of right-angled triangles.

Thirdly.—*To measure an inaccessible Distance in an oblique Angle, where a right Angle cannot be obtained by reason of some Impediment on the Ground.*

At the first station, from which the distance is required, place the instrument; then set up a staff, in any attainable direction, to any distance at pleasure (the more distant the better). The instrument being set, with its base in direction to the staff, with one of the moving limbs take the angle of the object, and with the screw fix it thereto. This done, move the instrument in the direction of its base (being between the first station and staff set up) to any certain distance (say 50 yards or measures), as a second station. From this second station again take the angle of the object, and thereto fix the other moving limb; this done, the distance both from first and second station, as also the bases and perpendiculars thereto, will thus readily be seen. Set the perpendicular at random to any height; move the same till the upper point intersect the upper limb, or that most distant from the base; then read off on the parallel the divisions parallel to the base subtended between the two hypothenuses or limbs; if this distance or division be equal to the

the distance measured on the base line (*i. e.* 50), then the distance of the object from both stations will be shown on the two limbs, as will also the base and perpendicular on the respective lines. If the divisions on the parallel do not agree with the distance measured, the perpendicular must be altered till that division be shown, when the required distance will be given. This is a case of our first problem.

Fourthly.—*To level or measure the Altitude of any Object.*

It is only necessary to set the plane of the instrument vertical instead of horizontal by means of the joint under the instrument, whence it is evident every case may be known as on the horizon; and to level, it is only requisite to set the spirit-level at the back of the instrument, the base line and every object cut by the same will be level thereto.

Fifthly.—*To take Angles or Altitudes at Sea, where the Instrument cannot be made stationary.*

For this purpose, it is first requisite to change the sight a, fig. 1, and substitute the one b, fig. 2; which being firmly fixed and adjusted at right angles with the upper limb, it is evident that when by reflection any object is brought to coincide on the mirror, at the extremity of the base line, with another object seen in the direction of such base, the angle will then be known, being double what the upper limb denotes on the arch; to which true angle, or its double, the lower limb may be fixed, leaving the one with the mirror again at liberty to take another observation and angle at any distant place or time; which being so taken, this limb may be also moved and fixed to double its apparent angle, and the altitude or distance be then determined, by setting the perpendicular and parallel as in other common cases on land.

From this mode of determining distances, as the use of calculations and of tables of sines and tangents are superseded, it is presumed that much convenience will arise to the unlettered who may have occasion to use it, and thereby the errors of calculations will be avoided.

As well as the before-mentioned purposes to which the instrument applies, it is presumed there will be found other

things

things which it will perform, some it is hoped useful and some amusing, amongst which may be enumerated multiplication, division, rule of three, double rule of three, &c.; determining the area or sides of any sort of triangle from any proper data; determining the inscribing or inscribed circle of any triangle, square, or polygon; showing a mean proportional between two numbers, &c.

It is presumed that an instrument, if perfectly made, on a large scale, would be found very useful and accurate in various practical calculations, as well for making them as for proving them after made in figures.

The following are specimens of the manner of calculating by this instrument:

First Question.—*If £.100 in 12 Months produce 80 Shillings Interest, what will £.200 produce in 18 Months; and also, what will it produce in 12 Months?*

On the base line of the instrument set £.100. On the perpendicular set 80, for shillings interest. Then bring the lower limb to intersect, which angle will then be, as per question, equal to 12 months at all places on the base: having so fixed the lower limb, move forward the perpendicular till it intersect the lower limb at the height 12 on the perpendicular; then raise the perpendicular to 18, and to the extremity thereof fix the upper limb to intersect, which angle will then be in proportion as 18 to 12 to the lower limb, being equal to the different times. The limbs being so fixed, it is only requisite to move the perpendicular to 200 on the base; and, raising the perpendicular till it intersect the upper limb, you will have thereon the answer, 240 shillings; and at the same time, at the intersection on the lower limb, 160, being the interest for 12 months only.

Second Question.—*To determine the inscribed or inscribing Circle of any Polygon, the Side being given; for example the Hexagon, whose Side is 100 Feet.*

Set one of the limbs to half the angle included in the required side of the hexagon (*i. e.* 30 degrees), then set the perpendicular to the height of half the side given, being as per question 50. Then move the perpendicular till the extremity

tremity intersect the limb before set, on which, at such intersection, will be denoted the radius of the inscribing circle, and at the same time may be seen on the base the radius of the inscribed circle.

Third Question.—*To find a mean Proportional between* 600 *and* 200.

This depends on the well-known Property of a right-angled Triangle.

Set the perpendicular on the base line at the distance of half of the difference of the two numbers (*i. e.* $\frac{600-200}{2}$); this done, raise the perpendicular, and move either of the limbs till the extremity of the perpendicular intersect thereon at half the sum of the numbers, being 400. This done, the height of the perpendicular will show the proportional required, being 347.

Note. On the plate in which the perpendicular slides, will be found Nonius's for subdividing the divisions on the base or perpendicular, into ten divisions.

Reference to the Engraving of Mr. Salmon's Geometrical Quadrant and Staff, (Plate IV.)

Fig. 1. represents the face of the quadrant, on which A is the fixed base line; B, the moveable perpendicular; C, the upper limb; D, the lower limb; E, the arc; F, the nut, which moves the perpendicular by means of a rack and pinion.

G, a spring to keep the perpendicular steady; H, a screw for fixing the joint of the staff; *a*, the eye-piece, or sight, with a small hole in its centre; I, I, I, the sights for direct vision, consisting of only a small slit in each. When objects are to be viewed by reflection, as with a Hadley's quadrant, the sight *a* at the centre is taken off, and the sight *b* with a mirror, shown at fig. 2. on rather a larger scale than the former, must be substituted.

Fig. 3. K is the staff, the mode of applying which to support the instrument when in use, is shown by the same letters in the other figures; L is the screw by which the staff is fixed firm in the ground.

Fig. 4.

Fig. 4. represents the back of the quadrant; MM are the screws by which the upper and lower limbs are fastened after taking an observation.

O, a spirit-level; H, the tightening-screw for the joint before noticed; P, the socket attached by its joint and tightening-screw to the back of the quadrant; the staff K is screwed into the above socket.

Fig. 5. shows the practical method of using the instrument for determining the distances of the objects Q and R from the two stations S and T, at which the instrument is to be successively placed, and used as before described.

Fig. 6. is the diagram referred to at page 164.

Fig. 7. represents the mode of applying the tightening-screw H, in fig. 1 and 4, by means of the semi-circular spring, inclosing the cylindrical stem, or neck of the joint.

XXVII. *A simple Way for determining the exact Time of Noon; also a Way to obtain a Meridian Line on a small Scale. By* Ez. Walker, *Esq.*

To Mr. Tilloch.

SIR, Lynn, July 12, 1806.

Although the following method of finding the exact time of noon has not the merit of much originality to recommend it, yet it may, I trust, prove acceptable to the young student in astronomy, who may not have the means of procuring expensive optical instruments for solving this fundamental problem.

Near the top of a room facing the south fix a plate of metal with a circular hole in it, for the sun to shine through from eight or nine o'clock in the morning until three or four in the afternoon; then, by means of a line and plummet, determine the point upon the floor which is directly under the centre of the hole, and from that point, as a centre, draw several concentric semicircles. Having made choice of some clear day near the summer solstice, make the room nearly dark, and about three or four hours before and after noon mark the points where the northern as also the southern limb

limb of the sun's image crosses those semicircles, and there will be several curves included between those points, through the middle of which a right line being drawn from the centre of the semicircles is a meridian line. After the line has been drawn in this manner, it must be examined by succeeding observations, and corrected if necessary, by which means a line may be drawn exceedingly near the true meridian, as appears from my former paper on this subject *.

Practical Observations.

1. The metal plate, which may be about one-fifth of an inch thick, must be placed parallel to the axis of the world, that the sun's rays may pass perpendicularly through it when he is in the equator.

2. The aperture need not be more than one-fifth of an inch in diameter, if it be countersunk on both sides, to admit the sun's rays to flow through it at the distance of three or four hours before and after noon.

3. If the surface of the floor on which the observations are to be taken for finding the meridian be not sufficiently even, the floor may be covered with new boards, taking the greatest care that they are laid down perfectly horizontal from east to west. After the line has been correctly drawn, and the north and south ends of it marked upon the walls of the room, the boards may be taken away and others laid down to draw the lines upon.

A meridian line, upon a small scale, but sufficiently correct for regulating clocks and watches, may be had by the following method:

Let a stone with an even surface, about three feet long and two broad, be fixed horizontally upon a brick or stone pillar at a convenient height for observation, with one of its ends facing the south. Near the middle of this end of the stone fix a gnomon, in a direction perpendicular to the horizon. This gnomon, which should be made of a strong bar of metal, must have a small aperture made through it, for the sun's rays to flow through early in the morning and late in the evening. From that point, as a centre, which is

* Philosophical Magazine, vol. xxiv. p. 289.

directly

directly under this aperture, draw several concentric semicircles, and fix the meridian line according to the preceding directions. The gnomon should have several other holes made through it in a line perpendicular to the horizon, that the sun's rays at noon, flowing through some of them, may fall near the north end of the stone at all seasons of the year; for if only one hole be used, the sun's image will fall near the centre of the gnomon in the summer, and in the winter it will be thrown far beyond the north end of the stone.

I am, sir, your humble servant,

EZ. WALKER.

XXVIII. *On the Satellites and Belts of Jupiter.* By *Mr.* JOHN SNART.

To Mr. Tilloch.

London, July 18, 1806.

SIR,

THE salutary effects of the most tempestuous weather are so obvious that most persons have observed them, and whatever disagreeable effects we may encounter while these august operations are going on, we are fully compensated for in the issue.

Friday last (11th instant) was a very tempestuous day: however, about ten o'clock P. M., after our atmosphere was pretty much attenuated by the discharge of vast quantities of rain, much thunder, lightning, &c.; and Jupiter being retrograde in longitude $9^s\ 1°\ 21'$, *i. e.* in $1°\ 21'$ of Capricorn, right ascension 272°, and consequently in that part of the heavens which is about south-south-east of London, my attention was called, by an acquaintance of mine, to a sight which afforded me a great deal of pleasure, viz. "a view of the third and fourth satellite of Jupiter by the naked eye." But though professor Beauchamp, I think, was so remarkable for the singular acuteness of his vision, that he could discern these auxiliaries by his natural optics, I was apprehensive that I might be deluded by some stars of the 7th or 8th degree of magnitude. Therefore, to prove the identity of these secondary planets, I took a good night telescope to examine the primary and its satellites, as well as to

to observe if there were any small stars in its vicinity; knowing that there are many nebulæ belonging to that part of the galaxy, or milky way, which runs through the preceding constellation Sagittarius, and by the precession of the equinox border upon the sign of ♑; but could not discern any: for the two satellites I had observed were in the places I saw them by my naked eye, as well as the two other lesser ones; that is to say, one of the outer ones was on the eastern and the other three were on the western side of their primary, thus:

I did not take particular notice of the belts at this time, because I was too much interested with the novelty of the satellites: but an idea has often occurred to me respecting the *rationale* of their mutations (*i. e.* of the belts) which I think may (at least analogically speaking) elucidate the phænomena of their fluctuation. For instance: when the sun is about to pass from either hemisphere into the other, the rarefaction of vapours consequent upon his presence causes so great a commotion in the atmosphere as to produce a continual current of wind, and to carry the clouds in one direction throughout most part of the torrid zone of our earth (called a monsoon, or general trade wind, &c.). Now, if we were to be elevated to a great height above our atmosphere, it is plain, I think, that this current must give the clouds a lineal direction, by which they would assume the appearance of one or more belts or girdles passing across the equatorial parts of the earth, and which belts would change their form and dimensions to an aërial observer in six months in a similar manner to those of Jupiter in $5\frac{3}{4}$ years (equal to half a year in that planet): I therefore think it but fair reasoning to suppose that Jovian trade winds, or something equivalent thereto, carrying his clouds in an equatorial direction, produce these mutations on the face of that planet which we call belts, and for which phænomena I have never seen any colourable conjecture. If these ideas meet your approbation, and you think them worthy of insertion, they are much at your service.—From yours respectfully,

Southwark Philosophical Society,
215, Tooley-street.

JOHN SNART.

XXIX. *Me-*

XXIX. *Memoir upon the Germination and Fermentation of Grains and Farinaceous Substances. By Messrs.* VAUQUELIN *and* FOURCROY *.

SIX years have now elapsed since we undertook a course of extensive inquiries upon vegetation, germination, and fermentation, in the laboratories of the Museum of Natural History. Although our experiments were very numerous a long time ago, we did not wish to publish them until we might consider our labours on the subject as completed. In spite of all our exertions this period has not yet arrived; the multiplicity of our experiments, however, has presented us with several facts, which, from their novelty, must be useful to the arts and sciences dependent on them. Besides, as several chemists are about to publish memoirs and experiments upon the same subject, we thought it advisable to publish our observations in their present shape. We shall commence, therefore, by an analysis of the principal alimentary grains, and by the experiments we had made upon their germination and fermentation. We shall give at the conclusion our labours upon the chemical phænomena of vegetation.

§ I. *Analysis of the Farina of Wheat.*

The water in which this farina has been macerated in equal quantities for six hours, clears very slowly; it is colourless, of a mild insipid taste, with the smell of bruised green corn; it becomes frothy by agitation; it does not redden turnsole paper, and it is not acid like barley water; it is precipitated by gall-nuts, by the acids, and particularly by the oxymuriatic acid; almost imperceptibly by the oxalate of potash, and not at all by lime water. It does not contain phosphate of potash, like the water which has been used in the maceration of garden beans.

This liquor speedily becomes sour, and even during filtration; it precipitates yellowish flakes by means of heat; reduced to one-half by evaporation, it becomes a little saccharine;

* From *Annales du Muséum d'Histoire Naturelle*, vol. vii.

rine; evaporated further, it is of a golden yellow, saccharine, acid and bitter, and becomes as thick as a strong solution of gum. In this second evaporation it forms on its surface a slender flexible pellicle of yellowish flakes; it deposits upon the vessel containing it, a white hard crust of phosphate of lime.

When thus thickened, the liquor is not disturbed by water; it is precipitated by the alkalis in a small quantity; abundantly by gall-nuts, by oxalate of ammonia, and by the acids. Alcohol coagulates it into a white, gluey, membranous, glutiniform substance, which, being evaporated, leaves a little deep yellow saccharo-acid matter.

The substance precipitated by alcohol is at first white and dry, softens and changes to brown on losing the alcohol; it then becomes semi-transparent, mild, and nauseous; at last it dries in the air, and is hard, brittle, and transparent, like strong size; when burning it bubbles up with a white and fetid smoke, and leaves plenty of charcoal.

It results from these experiments, that cold water makes of the farina of wheat a frothy substance precipitable by acids and gall-nuts, and which also sours, dissolves then more abundantly, and dissolves at the same time more of the phosphate of lime: it is analogous to gluten. It is united to a little mucilage, with a very small quantity of saccharine matter.

§ II. *Analysis of the Gluten of Wheat.*

Fresh gluten, well washed and very pure, macerated a long time in a little distilled water, renders it opake, leaving in it a substance minutely suspended, which does not separate from it: repeated filtrations clarify it. The clear water is frothy; by infusion of galls it precipitates yellow flakes; by oxymuriatic acid it precipitates white flakes. Thus the gluten of wheat is soluble in cold water.

This solution when heated becomes muddy, deposits yellowish flakes, and retains them in spite of long ebullition.

The gluten placed in the oxymuriatic acid becomes soft quickly, seems to dissolve, and afterwards coagulate into yellowish white flakes, which become transparent and greenish upon drying; placed upon burning charcoal it crackles, ex-

haling oxymuriatic acid, and afterwards has all the appearances of common gluten.

It dissolves very freely in concentrated acetic acid, which it renders turbid, and from which it may be separated by means of the alkalis, with all its properties, even after a lapse of many years. This fact is already well known to chemists.

Plunged into water at the temperature of 12° (53·6 Fahr.), this gluten melts, bubbles up to the surface, becomes sour and fetid, and exhales carbonic acid gas. The water filtered, and not clarified, reddens turnsole paper very strongly; is soon precipitated and clarified by the acids; the oxymuriatic acid produces an abundant precipitate, if used in great quantity: it is precipitated also by infusion of gall-nuts and by the fixed caustic alkalis, which disengage ammonia from it. The latter, precipitated by the alkalis, is dissolved in plenty of water.

The water of fermentation of the gluten (one pound with three ounces of white sugar) converted sugar into good vinegar, without either fermentation, effervescence, or contact of the atmosphere.

The gluten already fermented, put a second time into water at the temperature of 12 degrees (53·6 Fahr.) ferments again, disengages carbonic acid, is weakly acidified, and its acidity is not increased at the end of three or four days. The water decanted, and by this time fetid, reddens tincture of turnsole but slightly, and precipitates it; it becomes turbid by ammonia, the acids, infusion of galls, and the oxalate of ammonia; it deposits gluten by an excess of potash, exhaling an ammoniacal vapour.

After this second fermentation, which had formed ammonia and saturated the acid, the gluten becomes of a violet purple colour, forms at the surface of the water a pellicle of the same colour, becomes very fetid, passes afterwards to a blackish gray, and soon exhales the same odour with putrefied mucous membranes. At this period the water which floats above is blackish and muddy; it browns the nitrate of silver; blackens that of mercury at the minimum of oxidation by losing its own colour; becomes milky and inodorous by

by the oxymuriatic acid, and is no longer precipitated by infusion of galls.

After three months' putrefaction (March, April, and May) the gluten had a brown colour, and exhaled only a weak smell, but presented a great diminution in volume and mass. Separated, and then submitted to desiccation, it dried into lumps, the smell of which resembled that of the earth of burying-places: it softened under the finger like wax; it melted and burned with a flame and a smell like fat, yielded very little carbon, and dissolved in alcohol, to which it gave a brown colour; the portion not dissolved was dry, pulverulent, inodorous, insipid, and very like the ashes of charcoal; it burned with the sharp smell of wood, without ammonia, and left reddish gray ashes, in which iron and silex were found.

In this putrid decomposition of gluten azote is united to hydrogen, and a portion of carbon to oxygen, in order to form ammonia and carbonic acid. The carbon united more abundantly to hydrogen had produced the fat; and the principles superabundant to the formation of the carbonic acid, ammonia and fat, remained combined in a state something like that of a ligneous body.

§ III. *Analysis of Barley.*

Good wholesome and fresh ground barley contains almost always the acetic acid completely formed, and an animal matter more abundantly soluble in water than that of the farina of wheat, on account of the presence of the acid. Some barleys are not acid at all.

The water in which the farina of barley is diluted in equal volume, forms a thick, gluey, mucilaginous soup; when clarified, it is of an amber colour; its surface becomes brown, and the colour fades by degrees. After the departure of the acid, the water in which the barley is dissolved remains milky, and does not clarify, except by repeated filtrations. When drawn off, this water clarifies by itself, and becomes purple. It is very acid, and very nauseous; it contains an acid formed by fermentation, and an animal matter in large quantity, which the acid renders soluble.

The last solution of the barley contains no more saccharine matter; it however experiences the acetic fermentation, is precipitated purple by gall-nuts, white by the acids and the alkalis, which redissolve the precipitate, and it is precipitated green by the prussiate of potash. The substance which thickens the different waters which had washed the barley, is very analogous to the gluten of wheat.

The above waters, heated to 60° (140° of Fahr.), become muddy, deposit very abundant yellowish gray flakes, and yield red pellicles, brown at their surface. These flakes and pellicles, when burnt, leave a fifth of their weight of phosphate of lime and magnesia; they do not cause the saccharine matter to ferment. The liquor, after acquiring the consistence of syrup by evaporation, mixed with sugar, ferments no longer, so that the vegeto-animal matter of barley, dissolved in water, or already altered by fermentation, is not the ferment of sugar.

The syrup of barley diluted in three or four parts of water, and the mixtures of the precipitates and of sugar, fermented and became sour, but without showing any appearance of alcohol; the vegeto-animal matter of the barley and sugar contributed to the formation of the acid. These syrups always preserved their saccharine matter and their vegeto-animal viscous matter. The sugar, being greatly diminished by these operations, may thus be acidified without being first converted into wine, and without the contact of the air.

Barley water thickened into syrup is brown, sweet, and acid; it is abundantly precipitated by gall-nuts, oxymuriatic acid, and the alkalis. Alcohol precipitates from it a very abundant brown matter, which furnishes a good deal of phosphate of lime by combustion.

These phænomena, belonging to the solution of a vegeto-animal matter, explain why the vinegars produced from grains are less agreeable and less decomposable than those produced from wine; and also why they precipitate by gall-nuts, ammonia, and the acids, while wine vinegars do not present these characteristics. We see also by this how the vinegar of grains is better preserved after a slight ebullition,

 recommended

recommended by Scheele, who, no doubt, meant this kind of vinegar alone.

Barley, exhausted by washings in cold water, when digested a few days in alcohol, gives a yellow colour to it; when distilled this alcohol contracts the smell and taste of spirits distilled from grain; it leaves a thick oil, yellow, brown, and greenish, which is even got from barley not diluted, and which is then mixed with the saccharine substance. This discovery accounts for the bitterness of the water of peeled barley, and for the necessity of throwing away the first decoction of this grain.

One hundred parts of the farina of barley, macerated for thirty hours in alcohol, gave it a golden yellow colour, and the sharp taste of spirits distilled from grain. This alcohol is precipitated by means of water, and becomes much more odorous. When distilled, it preserved its smell, and left eight grammes of an oily matter, yellow, brown, and bitter, and which condensed into a species of soft beer. This matter contained sugar, which the water had separated from it, and was reduced to nearly an eighth of its primitive weight, in such a manner, that the oil of the barley only made a hundredth part of the grain.

This oil becomes clotty like olive oil; it volatilizes on red hot iron; it burns like any other fat oil, and forms a soap with alkalis. It is manifestly this oil which gives a bitter rancid taste to barley bread, and the disagreeable smell and taste which belong to spirits from grains. We may observe, that this fixed or fat oil is not dissolved in alcohol, but by employing the latter in very great quantity.

The farina of barley, treated twice by alcohol, was washed four times with water; the waters evinced the same appearances as already said, only the vinegar which they yielded was of a lively taste and smell; this certainly depends upon the alcohol which remained in the farina.

The husks, steeped in water, placed in fine linen and agitated in plenty of water, deposited starch; there remained in the linen a sort of gray gluten, flaky and a little elastic, which gave the same products, when exposed to the fire, as

that of the farina, the incinerated charcoal of which furnished phosphates of lime and magnesia, quicklime and iron.

According to these experiments barley contains, 1st, fat oil, capable of concreting, weighing one hundredth; 2dly, sugar, forming about seven hundredths; 3dly, starch; 4thly, an animal matter, partly soluble in the acetic acid and partly consisting of glutinous flakes; 5thly, phosphates of lime and magnesia; 6thly, silex and iron; and, 7thly, acetic acid, which, however, is not in all barleys, but which is often enough found to deserve notice.

[To be continued.]

XXX. *Proceedings of Learned Societies.*

ROYAL SOCIETY OF LONDON.

JUNE 26. The Right Honourable Sir Joseph Banks, Bart. President, in the chair.—A paper by Mr. Gilpin, the register of the society, was read, containing very interesting and curious observations on the dip and variation of the magnetic needle, made at his apartments in Somerset-house, under the direction of Mr. Cavendish, from the year 1786 to 1806, and reduced to five analytical tables, of which Dr. Gray, the secretary, made a very complete and perspicuous summary.

An account of an analysis, by Mr. Smithson Tennant, of a kind of native iron found at the Cape of Good Hope, was read. This metal consisted of an alloy of nickel and iron in the proportion of one of the former to ten of the latter. It yielded plumbago when treated with acid. The specimen subjected to our author's experience was about six inches long, four and a half broad, and two thick, and was supposed, like the stones lately discovered, to have fallen from the clouds.

A letter from a gentleman in Somersetshire to Mr. Tennant, on the latitude of certain stars, was likewise read.

Dr. Herschel furnished a paper as a summary of, and

sequel to, his former papers on the figure of Saturn. The present observations are designed to illustrate and correct this astronomer's former opinions, as well as the outline of the figure of Saturn, given in the Philosophical Transactions of last year *. He now considers the diameter of that planet to be much greater at the equator than formerly, but still admits that it is much flatted at the poles.

The society then adjourned till Thursday, the 6th of November next.

SOCIETY OF ANTIQUARIES.

June 26. The right honourable the earl of Leicester, president, in the chair.—Mr. Malcolm furnished notes of the registers and inscriptions found in the church of St. Helens, London.

Lord Egremont produced a copy of another order issued in the 5th of Henry VIII., directing and arranging the equipage and state splendour of the duke of Northumberland on his embarkation for France. The publication of these historical documents must tend to render luxury and pageantry more contemptible.

Three numbers of the engraved drawings taken from the paintings discovered in St. Stephen's chapel, Westminster, in 1800, were presented to the members; after which the society adjourned till Thursday, the 6th of November.

IMPERIAL ACADEMY OF GENOA
(*Formerly called the Institute*).

The two classes of this academy, according to their new regulations, are in future to hold a meeting every month: the class of Physical Science on the 1st, and that of Moral Science on the 15th, of every month.

POLISH SOCIETY OF THE FRIENDS OF LITERATURE, AT WARSAW.

On the 17th of May last the above society held their public meeting. Bishop Albertrandi, as president, opened the business with an appropriate discourse, in which he paid some elegant compliments to prince Alexander Sapihea,

* Philosophical Magazine, vol. xxiii. p. 153.

 whose

whose return from his travels to his native country was formerly announced, and whose researches and collections made during his absence will be of great service to the study of antiquities and natural history. Abbé Stasic read an account of his geological tour through the whole of Poland, undertaken with a view to extend the knowledge of the natural history of that country: the Karpat mountains were the particular objects of his inquiries.

XXXI. *Intelligence and Miscellaneous Articles.*

ERUPTION OF MOUNT VESUVIUS.

Extract of a private Letter, dated Naples, June 6.

On the 31st of last month we enjoyed the spectacle of an eruption of Vesuvius. A column of very black smoke rose from the crater about ten o'clock; flashes now and then burst from this column; at length the eruption appeared in a mass of flame of immense diameter, and occupying the whole vast extent of the crater. This mass was kept up by successive emissions of whitish flame, which, as it rose into the air, assumed a more intense red colour. Ignited or melted substances were projected with violence above this body of fire, and often fell beyond the circumference of the crater. At midnight there was not as yet any current of lava, but frequent rumblings were heard.

On the 1st of June the eruption continued the whole morning, and we resolved to visit the mountain the following night.

We set out at eight in the evening. We took horse at Resina, near the descent to Herculaneum, and proceeded towards the residence of the Hermit. The house in which he lives is situated near the southern peak of Mount Somma, being an easy ride of an hour and a half from Resina.

On leaving the hermitage, we proceeded across the valley which separates Somma from Vesuvius, and is known by the appellation of Alrio del Cavallo. It is of no great depth, being almost entirely filled with the lavas of successive

sive eruptions, piled one above another. At length we reached the foot of Vesuvius, where we left our horses, and began to ascend on foot.

The declivity is very steep, and difficult of ascent, on account of the moveable nature of the ground on which you walk, being nothing but a mixture of ashes and fragments of lava, without consistency. After great fatigue we reached the summit, and arrived at one of the edges of the crater.

We had been lighted the whole way by eruptions of the mountain, which were projected to a very great height. Violent rumblings, that were continually heard, added to the grandeur and the awfulness of the spectacle, which appeared much more beautiful and majestic from the point to which we had climbed with so much difficulty.

Suspended as it were on the brink of the crater, nothing interposed to prevent our view of the eruptions. We beheld immense masses of flame issuing almost from under our feet, rising above the clouds, and carrying with them, to the same height, showers of ignited stones, which generally descended, nearly in a perpendicular direction, into the very mouth of the crater; but, sometimes falling beyond its brink, rebounded around us, and rolled, red hot, down the declivity which we had climbed. Columns of fire, clouds of smoke, and showers of stones, succeeded each other, without interruption, accompanied by continual subterraneous noises; the bowels of the mountain seemed convulsed; the ground on which we stood shook, and threatened to sink beneath our feet. Never had we beheld a more melancholy image of the convulsions of nature; and notwithstanding the risk we incurred from the continual falling of the stones, we could scarcely be prevailed upon to leave it.

Our guides, who were better judges of the danger than ourselves, now became alarmed, and urged us to descend. The violence of the volcano had increased since we reached the summit; and the power that presides over the place seemed inclined to punish us for our audacity, and for having presumed to violate his tremendous abode.

We accordingly descended, and in a few minutes arrived at the Alrio del Cavallo. We were out of the reach of

danger,

danger, and were enabled to contemplate, without apprehension, the objects by which we were surrounded. What an admirable spectacle!—Over our head, the volcano, with its smoking lava rushing down the sides of the mountain; before us, the sea smooth and calm; the full moon illuminating with her mild beams the extremity of the horizon; the clouds and the smoke wafted around the summit of the mountain, and concealing, for a few moments, the vast conflagration, which appeared again more lively and more brilliant: this succession of lights and shades, this contrast of turbulence and tranquillity, this solitude in the midst of such a vast convulsion, produced a multitude of contrary impressions that cannot be described, but the recollection of which will never be erased.

We returned about four in the morning to Naples, having spent eight hours in the excursion.

On the second, the eruption continued the whole day with much greater violence than before; two currents of lava were formed; one of these stopped in the morning; the other, taking an eastern course, spread with great rapidity, and deluged the plain. As our excursion of the preceding night had not enabled us to form any idea concerning the progress of the lava, we set out again to observe this extraordinary phænomenon.

Passing through the villages of Portici, Resina, and Torre del Greco, we entered inclosures, consisting of vineyards and corn-fields, into which the lava had penetrated: we approached the current, and I was surprised to find the progress of the lava so different from the conception I had formed of it.

I had always imagined that the substance of the lava, resembling melted glass, ran in the same manner, and advanced uniformly like a river of fire; and indeed it is extremely probable that in a great number of eruptions it actually exhibits this appearance. On the present occasion, I saw nothing but an accumulation of stones, some of which were of prodigious magnitude, heaped one upon another, to the height of fifteen or twenty feet, and about half a mile in breadth. This formidable mass advanced slowly,

slowly, following a progression produced solely by the falling of the different bodies, between which there was no adhesion, and which, in obedience to the impulsion they had received on issuing from the crater, rolled from the most elevated point, and covered the surface of another stratum. In this manner the stones rolled over, one upon another, till the front rank having attained the same height as that which produced it, began in its turn to pour down the ignited bodies that came tumbling upon it.

All this intestine motion was accompanied with a noise resembling the decrepitation of salts, but much more loud and brisk. The fire was fed by various combustible matters, as sulphur, bitumen, and metals, which might be known by their flames; but there was no appearance either of complete fusion or of the commencement of it. The stones resisted the pressure of a stick, which I several times endeavoured, but in vain, to thrust into them.

Meanwhile the devastation occasioned by the progress of this torrent presented a horrid spectacle. The trees which supported the vines, and the vines themselves, were burned by the extreme heat of this mass of matter, even before it reached them; and the bright and clear light produced by their combustion, indicated the exact contour of the progress of the lava. The walls of inclosures, and of houses, calcined by the heat, crumbled to pieces before this moving mountain, or were thrown down by the force of the impulsion. Sometimes, however, instead of overturning an obstacle, the lava turned aside, and left it standing: for this variety of action it is impossible to assign any reason.

After we had contemplated this dismal and astonishing sight, we went up to the convent of the Camaldulenses, situated on a kind of peak, of considerable height, that overlooks the whole plain, which extends from the south to the west, from the foot of Mount Vesuvius to the sea. This building has hitherto been spared, as well as the thick wood in which it is embosomed. It is one of the nearest points to Vesuvius, and that from which you are best able to discover and trace the progress of the lava. It is the asylum to which the wretched inhabitants of the desolated plain have often

often fled, with their most valuable effects; to which they have driven their flocks, and conveyed their wives and children.

Here we staid a considerable time; our view extended over the declivity of Vesuvius, from which ran several currents of lava, that issued from the sides of the mountain; while enormous flames of fire, of which we had a nearer prospect the night before, darted continually from its summit. We had likewise a view of the plain, in which appeared the long windings of the rivers of fire. The reddish reverberation of the lava, and the conflagration in the plain, illumined the landscape. On every side appeared the image of desolation; but yet it exhibited a picture so splendid, a scene so magnificent, that the ravages with which it was attended were entirely forgotten in the contemplation of its picturesque and poetic beauty. In short, when my mind figures to itself those fiery torrents, the motion of the lava, the subterraneous thunders, those continual hissings—so many wonders, so many subjects of grief and admiration—I should think that a dream had deceived me, if the imagination, which produces such dreams, were capable of creating images so awful and so grand.

On the 3d, the eruption continued, and the lava still advanced; the thunder was louder and more frequent than the preceding day. In the evening the flames shot to a still greater height, attracting the electricity of the air, and of the clouds, which emitted splendid flashes.

On the 4th, the eruption was less violent.

On the 5th, Vesusius began to throw out ashes; which, we are assured, announces the conclusion of the eruption.

SINGULAR METEOR.

A very singular and brilliant meteor was seen by many of the inhabitants of London, in broad day-light, on Thursday afternoon, the 17th of July, just about eight o'clock; passing in the southern and western part of the hemisphere, from about south-east to north-west: it seemed about one fourth of the apparent diameter of the moon, but more brilliant than Venus ever appears, and moved with very great swiftness, nearly in an horizontal direction, leaving a conical

tail

tail of light and sparks behind it. We entreat our correspondents in the country to communicate any observations which they can collect on the course of this large and singular meteor, and of its fall, if it should have been seen to fall in Britain.

PIT COAL.

A letter from Salisbury, in Virginia, North America, states, that the strata of coal there are near the surface of the earth, and very thick. One stratum was lately discovered of the astonishing thickness of 42 feet, and so near the surface, that the earth is merely taken off, and the coal dug out without undermining.

The last winter was the mildest and driest ever known in America.

MUNGO PARK.

In the beginning of the present month (July) a letter was received from the River Gambia, stating, that Mungo Park, the traveller, and his retinue (two or three excepted), had been murdered by the natives in the interior of the country. This news is stated to have been verified by the arrival of the persons who escaped the massacre, at Widah.

LIST OF PATENTS FOR NEW INVENTIONS,

For the Months of June and July 1806.

To Ralph Dodd, of Change-alley, in the city of London, engineer; for a method of applying steam for the forcing and raising of water, heavy bodies, and working machinery, in a more simplified manner than has hitherto been practised. Dated June 6.

To Edward Massey the younger, of Newcastle, in the county of Stafford, nautical instrument-maker; for certain improvements in the construction of an instrument or apparatus for taking soundings at sea, whereby the same will be much more simplified; for which instrument or apparatus he has already obtained letters patent, bearing date the 24th of March 1802. Dated June 6.

To William Deverell, of Charles-street, Blackfriars-road, in the county of Surrey, engineer; for certain improvements in the mode of giving motion to hammers, stampers, knives, sheers, and other things, without the application of wheel, pinion,

pinion, or any rotative-motion, by means of various powers now in common use, whereby much labour and expense will be saved. Dated June 6.

To Lawrence Gwynnet, of Christ's Hospital, in the city of London, gentleman, and Peter Noble, of the London-road, in the county of Surrey, engine-maker; for certain improvements in chain and common pumps, whereby the latter will act as a fire-engine for sea and land purposes. Dated June 6.

To Edward Heard, of London, chemist; for certain means of obtaining inflammable gas from pit coal, in such a state that it may be burned without producing any offensive smell. Dated June 12.

To Samuel Phelps, of Cuper's Bridge, Lambeth, in the county of Surrey, esq.; for his improved method of making kelp, barilla, or other vegetable or mineral alkali, by fermentation and other means, in addition to combustion. Dated June 17.

To William Leicester, of Piccadilly, engineer; for an improved rotary motion or engine to communicate power to machines. Dated June 19.

To William Clark, of Cerne Abbas, in the county of Dorset, clockmaker, and Joseph Bugby, of Yeovil, in the county of Somerset, schoolmaster; for certain improvements in a machine for spinning hemp, flax, tow, and wool. Dated June 19.

To Thomas Bourne and William Chambers, scale-beam makers, and Chester Gould, mechanic, all of Birmingham, in the county of Warwick; for a machine or engine for roasting meat by the power of steam, and for other purposes where small powers are necessary. Dated June 24.

To John Davenport, of Langport, in the county of Stafford, glass-maker; for a method of ornamenting glass in imitation of engraving or etching, by which means borders, cyphers, coats of arms, drawings, and the most elaborate designs, may be executed in a style of elegance hitherto unknown. Dated July 4.

To John Curr, of Sheffield, in the county of York; for a method of applying the cables of ships and vessels upon the windlasses,

windlasses, capstans, or drums thereof; whereby the necessity of conveying away the loose ends of the said cables is prevented; by which methods the cables are more commodiously applied to their work, and more certain of bringing ships and vessels safe to anchor. Dated July 4.

To Bryan Donkin, of Fort Place, Bermondsey, in the county of Surrey, engineer, and Henry Maudslay, of Margaret-street, Cavendish-square, in the county of Middlesex, engineer; for a new and simple method of combining wheel-work together, so as to produce any required proportion of velocity between the weight and the first mover. Dated July 24.

To Charles de Berenger, of Hart-street, Bloomsbury, in the county of Middlesex, artist, in consequence of a communication made to him by a certain foreigner residing abroad; for a certain animal substance, and method of preparing and manufacturing the same; whereby the said substance becomes applicable as a substitute for horse and other hair now used for the stuffing of cushions, mattresses, carriages, sofas, chairs, &c., and all other purposes for which flocks, wool, or hair are now generally applied. Dated July 24.

To Henry Fourdrinier, of Sherborne-lane, in the city of London, stationer and paper-maker, in consequence of a communication made to him by a certain foreigner residing abroad; for a method of making a machine for cutting paper on a different principle from any hitherto used. Dated July 24.

To the same, in consequence of a similar communication; for a method of making a machine for manufacturing paper of an indefinite length, laid and wove with separated moulds. Dated July 24.

To John Lamb, of the State of New York, in North America, at present residing in King-street, in the city of London, merchant, in consequence of a communication made to him by a certain foreigner residing abroad; for certain improvements in and upon a machine, or machinery, for extracting fresh water from the salt water of the ocean, (by distillation) and other purposes, at sea or elsewhere. Dated July 25.

METEORO-

METEOROLOGICAL TABLE,
By Mr. Carey, of the Strand,
For July 1806.

Days of the Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
June 27	56°	57°	49°	29·82	10°	Showery
28	50	61	50	30·00	51	Fair
29	56	68	59	·20	56	Fair
30	69	65	59	29·90	0	Rain
July 1	59	64	57	30·00	46	Cloudy
2	60	66	61	·15	51	Fair
3	63	72	66	·01	54	Fair
4	63	68	55	29·89	25	Cloudy
5	60	69	60	·90	50	Fair
6	60	55	56	·81	0	Rain
7	59	64	58	·52	0	Rain
8	60	68	63	·62	25	Cloudy
9	60	74	64	·92	24	Cloudy
10	65	77	68	30·08	47	Fair
11	74	72	60	29·88	0	Rain, with thunder
12	63	71	65	30·00	56	Fair
13	66	76	68	29·90	47	Fair
14	68	68	64	·68	0	Showery
15	64	70	62	·80	35	Fair
16	60	71	55	·64	58	Fair
17	62	66	57	·70	0	Showery, with thunder
18	60	58	55	·76	0	Showery
19	55	66	55	·83	40	Fair
20	56	66	54	·93	45	Cloudy
21	55	66	54	·80	0	Showery
22	56	68	57	·78	46	Fair
23	57	70	56	·50	0	Showery
24	58	69	57	·61	0	Thunder, with great fall of rain
25	57	67	56	29·60	43	Fair
26	58	70	58	·59	50	Fair

N. B. The barometer's height is taken at noon.

Erratum.—Vol. xxv. p. 12. line 8., article *Electricity*, for *loaded* jar read *coated* jar.

XXXII. *Account of a Series of Experiments, showing the Effects of Compression in modifying the Action of Heat. By Sir* James Hall, *Bart. F.R.S. Lond. and Edin.*

[Concluded from p. 157.]

§ IX.

Application of the foregoing Results to Geology.—The Fire employed in the Huttonian Theory is a Modification of that of the Volcanoes.—This Modification must take place in a Lava previous to its Eruption.—An internal Lava is capable of melting Limestone.—The Effects of Volcanic Fire on Substances in a subterranean and sub-marine Situation, are the same as those ascribed to Fire in the Huttonian Theory.—Our Strata were once in a similar Situation, and then underwent the Action of Fire.—All the Conditions of the Huttonian Theory being thus combined, the Formation of all Rocks may be accounted for in a satisfactory Manner.—Conclusion.

Having investigated, by means of the foregoing experiments, some of the chemical suppositions involved in the Huttonian theory, and having endeavoured to assign a determinate limit to the power of the agents employed, I shall now apply these results to geology, and inquire how far the events, supposed antiently to have taken place, accord with the existing state of our globe.

The most powerful and essential agent of the Huttonian theory is fire, which I have always looked upon as the same with that of volcanoes, modified by circumstances which must, to a certain degree, take place in every lava previous to its eruption.

The original source of internal fire is involved in great obscurity; and no sufficient reason occurs to me for deciding whether it proceeds by emanation from some vast central reservoir, or is generated by the local operation of some chemical process. Nor is there any necessity for such a decision; all we need to know is, that internal fire exists, which no one can doubt, who believes in the eruptions of Mount Vesuvius.

Vesuvius. To require that a man should account for the generation of internal fire, before he is allowed to employ it in geology, is no less absurd than it would be to prevent him from reasoning about the construction of a telescope, till he could explain the nature of the sun, or account for the generation of light *. But while we remain in suspense as to the prime cause of this tremendous agent, many circumstances of importance with regard to it may fairly become the subjects of observation and discussion.

Some authors (I conceive through ignorance of the facts) have alleged, that the fire of Ætna and Vesuvius is merely superficial. But the depth of its action is sufficiently proved by the great distance to which the eruptive percussions are felt, and still more by the substances thrown out uninjured by some eruptions of Mount Vesuvius. Some of these, as marble and gypsum, are incapable, in freedom, of resisting the action of fire. We have likewise granite, schistus, gneiss, and stones of every known class, besides many which have never, on any other occasion, been found at the surface of our globe. The circumstance of these substances having been thrown out unaffected by the fire, proves that it has proceeded from a source, not only as deep, but deeper than their native beds; and as they exhibit specimens of every class of minerals, the formation of which we pretend to explain, we need inquire no further into the depth of the Vesuvian fire, which has thus been proved to reach below the range of our speculations.

Volcanic fire is subject to perpetual and irregular variations of intensity, and to sudden and violent renewal, after long periods of absolute cessation. These variations and intermissions are likewise essential attributes of fire, as employed by Dr. Hutton; for some geological scenes prove that the indurating cause has acted repeatedly on the same substance, and that, during the intervals of that action, it had ceased entirely. This circumstance affords a complete

* This topic, however, has of late been much urged against us, and an unfair advantage has been taken of what Mr. Playfair has said upon it. What he gave as mere conjecture on a subject of collateral importance, has been argued upon as the basis and fundamental doctrine of the system.

answer to an argument lately urged against the Huttonian theory, founded on the waste of heat which must have taken place, as it is alleged, through the surface. For if, after absolute cessation, a power of renewal exists in nature, the idea of waste by continuance is quite inapplicable.

The external phænomena of volcanoes are sufficiently well known; but our subject leads us to inquire into their internal actions. This we are enabled to do by means of the foregoing experiments, in so far as the carbonate of lime is concerned.

Some experiments, which I formerly* laid before this society and the public, combined with those mentioned in this paper, prove that the feeblest exertions of volcanic fire are of sufficient intensity to perform the agglutination, and even the entire fusion, of the carbonate of lime, when its carbonic acid is effectually confined by pressure; for though lava, after its fusion, may be made, in our experiments, to congeal into a glass in a temperature of 16° or 18° of Wedgewood, in which temperature the carbonate would scarcely be affected, it must be observed that a similar congelation is not to be looked for in nature; for the mass, even of the smallest stream of lava, is too great to admit of such rapid cooling. And, in fact, the external part of a lava is not vitreous, but consists of a substance which, as my experiments have proved, must have been congealed in a heat of melting silver, that is, in 22° of Wedgewood; while its internal parts bear a character indicating that they congealed in 27° or 28° of the same scale. It follows, that no part of the lava, while it remained liquid, can have been less hot than 22° of Wedgewood. Now, this happens to be a heat in which I have accomplished the entire fusion of the carbonate of lime under pressure. We must therefore conclude, that the heat of a running lava is always of sufficient intensity to perform the fusion of limestone.

In every active volcano a communication must exist between the summit of the mountain and the unexplored region, far below its base, where the lava has been melted,

* Edinburgh Transactions, vol. v. part i. p. 60—66

and whence it has been propelled upwards; the liquid lava rising through this internal channel, so as to fill the crater to the brim, and flow over it. On this occasion, the sides of the mountain must undergo a violent hydrostatical pressure outwards, to which they often yield by the formation of a vast rent, through which the lava is discharged in a lateral eruption, and flows in a continued stream sometimes during months. On Ætna most of the eruptions are so performed; few lavas flowing from the summit, but generally breaking out laterally at very elevated stations. At the place of delivery, a quantity of gaseous matter is propelled violently upwards, and, along with it, some liquid lava; which last, falling back again in a spongy state, produces one of those conical hills which we see in great number on the vast sides of Mount Ætna, each indicating the discharge of a particular eruption. At the same time, a jet of flame and smoke issues from the main crater, proving the internal communication between it and the lava; this discharge from the summit generally continuing, in a greater or a less degree, during the intervals between eruptions. (Fig. 41. represents an ideal section of Mount Ætna; *ab* is the direct channel, and *bc* is a lateral branch).

Let us now attend to the state of the lava within the mountain during the course of the eruption; and let us suppose, that a fragment of limestone, torn from some stratum below, has been included in the fluid lava, and carried up with it. By the laws of hydrostatics, as each portion of this fluid sustains pressure in proportion to its perpendicular distance below the point of discharge, that pressure must increase with the depth. The specific gravity of solid and compact lava is nearly 2·8; and its weight, when in a liquid state, is probably little different. The table shows that the carbonic acid of limestone cannot be constrained in heat by a pressure less than that of 1708 feet of sea, which corresponds nearly to 600 feet of liquid lava. As soon, then, as our calcareous mass rose to within 600 feet of the surface, its carbonic acid would quit the lime, and, assuming a gaseous form, would add to the eruptive effervescence. And this change would commonly begin in much greater depths,

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in consequence of the bubbles of carbonic acid, and other substances, in a gaseous form, which, rising with the lava, and through it, would greatly diminish the weight of the column, and would render its pressure on any particular spot extremely variable. With all these irregularities, however, and interruptions, the pressure would in all cases, especially where the depth was considerable, far surpass what it would have been under an equal depth of water. Where the depth of the stream, below its point of delivery, amounted, then, to 1708 feet, the pressure, if the heat was not of excessive intensity, would be more than sufficient to constrain the carbonic acid, and our limestone would suffer no calcination, but would enter into fusion; and if the eruption ceased at that moment, would crystallize in cooling along with the lava, and become a nodule of calcareous spar. The mass of lava containing this nodule would then constitute a real whinstone, and would belong to the kind called *amygdaloid*. In greater depths still, the pressure would be proportionally increased, till sulphur, and even water, might be constrained; and the carbonate of lime would continue undecomposed in the highest heats.

If, while the lava was in a liquid state, during the eruption or previous to it, a new rent (*de*, fig. 41.), formed in the solid country below the volcano, was met by our stream (at *d*), it is obvious that the lava would flow into the aperture with great rapidity, and fill it to the minutest extremity, there being no air to impede the progress of the liquid. In this manner, a stream of lava might be led from below to approach the bottom of the sea (*ff*), and to come in contact with a bed of loose shells (*gg*) lying on that bottom, but covered with beds of clay, interstratified, as usually occurs, with beds of sand, and other beds of shells. The first effect of heat would be to drive off the moisture of the lowest shell-bed in a state of vapour, which, rising till it got beyond the reach of the heat, would be condensed into water, producing a slight motion of ebullition, like that of a vessel of water, when it begins to boil, and when it is said to simmer. The beds of clay and sand might thus undergo some heaving and partial derangement, but would still possess the

 power

power of stopping, or of very much impeding, the descent of water from the sea above; so that the water which had been driven from the shells at the bottom would not return to them, or would return but slowly; and they would be exposed dry to the action of heat *.

In this case, one of two things would inevitably happen: either the carbonic acid of the shells would be driven off by the heat, producing an incondensable elastic fluid, which, heaving up or penetrating the superincumbent beds, would force its way to the surface of the sea, and produce a submarine eruption, as has happened at Santorini and elsewhere, or the volatility of the carbonic acid would be repressed by the weight of the superincumbent water (*kk*), and the shell-bed, being softened or fused by the action of heat, would be converted into a stratum of limestone.

The foregoing experiments enable us to decide in any particular case, which of these two events must take place when the heat of the lava and the depth of the sea are known.

The table shows, that under a sea no deeper than 1708 feet, near one-third of a mile, a limestone would be formed by proper heat; and that, in a depth of little more than one mile, it would enter into entire fusion. Now, the common soundings of mariners extend to 200 fathoms, or 1200 feet. Lord Mulgrave † found bottom at 4680 feet, or nearly nine-tenths of a mile; and captain Ellis let down a sea-gage to the depth of 5346 feet ‡. It thus appears, that at the bottom of a sea which would be sounded by a line much less than double of the usual length, and less than half the depth of that sounded by lord Mulgrave, limestone might be formed by heat; and that at the depth reached by captain Ellis, the entire fusion would be accomplished, if the bed of shells were touched by a lava at the extremity of its course, when its heat was lowest. Were the heat of the lava greater, a

* This situation of things is similar to what happens when small-coal is moistened, in order to make it cake. The dust, drenched with water, is laid upon the fire, and remains long wet, while the heat below suffers little or no abatement.

† Voyage towards the North Pole, p. 142.

‡ Philosophical Transactions 1751, p. 212.

greater

greater depth of sea would, of course, be requisite to constrain the carbonic acid effectually; and future experiments may determine what depth is required to co-operate with any given temperature. It is enough for our present purpose to have shown that the result is possible in any case, and to have circumscribed the necessary force of these agents within moderate limits. At the same time it must be observed that we have been far from stretching the known facts; for, when we compare the small extent of sea in which any soundings can be found, with that of the vast unfathomed ocean, it is obvious, that in assuming a depth of one mile or two, we fall very short of the medium. M. de la Place, reasoning from the phænomena of the tides, states it as highly probable that this medium is not less than eleven English miles *.

If a great part or the whole of the superincumbent mass consisted, not of water, but of sand or clay, then the depth requisite to produce these effects would be lessened in the inverse ratio of the specific gravity. If the above-mentioned occurrence took place under a mass composed of stone firmly bound together by some previous operation of nature, the power of the superincumbent mass, in opposing the escape of carbonic acid, would be very much increased by that union and by the stiffness or tenacity of the substance. We have seen numberless examples of this power in the course of these experiments, in which barrels, both of iron and porcelain, whose thickness did not exceed one-fourth of an inch, have exerted a force superior to the mere weight of a mile of sea. Without supposing that the substance of a rock could in any case act with the same advantage as that of an uniform and connected barrel, it seems obvious that a similar power must, in many cases, have been exerted to a certain degree.

We know of many calcareous masses which, at this moment, are exposed to a pressure more than sufficient to ac-

* "On peut donc regarder au moins comme très probable, que la profondeur moyenne de la mer n'est pas au-dessous de quatre lieues."—De la Place, *Hist. de l'Acad. Roy. des Sciences*, année 1776.

complish their entire fusion. The mountain of Saleve, near Geneva, is 500 French fathoms, or nearly 3250 English feet, in height, from its base to its summit. Its mass consists of beds, lying nearly horizontal, of limestone filled with shells. Independently, then, of the tenacity of the mass, and taking into account its mere weight, the lowest bed of this mountain must, at this moment, sustain a pressure of 3250 feet of limestone, the specific gravity of which is about 2·65. This pressure, therefore, is equal to that of 8612 feet of water, being nearly a mile and a half of sea, which is much more than adequate, as we have shown, to accomplish the entire fusion of the carbonate on the application of proper heat. Now, were an emanation from a volcano to rise up under Saleve, and to penetrate upwards to its base, and stop there, the limestone to which the lava approached would inevitably be softened without being calcined, and, as the heat retired, would crystallize into a saline marble.

Some other circumstances relating to this subject are very deserving of notice, and enable us still further to compare the antient and modern operations of fire.

It appears, at first sight, that a lava, having once penetrated the side of a mountain, all subsequent lavas should continue, as water would infallibly do, to flow through the same aperture. But there is a material difference in the two cases. As soon as the lava has ceased to flow, and the heat has begun to abate, the crevice through which the lava had been passing remains filled with a substance, which soon agglutinates into a mass far harder and firmer than the mountain itself. This mass, lying in a crooked bed, and being firmly welded to the sides of the crevice, must oppose a most powerful resistance to any stream tending to pursue the same course. The injury done to the mountain by the formation of the rent, will thus be much more than repaired, and in a subsequent eruption the lava must force its way through another part of the mountain or through some part of the adjoining country. The action of heat from below seems in most cases to have kept a channel open through the axis of the mountain, as appears by the smoke and flame which is habitually discharged at the summit during inter-

vals

vals of calm. On many occasions, however, this spiracle seems to have been entirely closed by the consolidation of the lava, so as to suppress all emission. This happened to Vesuvius during the middle ages. All appearance of fire had ceased for five hundred years, and the crater was covered with a forest of antient oaks, when the volcano opened with fresh vigour in the sixteenth century.

The eruptive force capable of overcoming such an obstacle must be tremendous indeed, and seems in some cases to have blown the volcano itself almost to pieces. It is impossible to see the mountain of Somma, which, in the form of a crescent, embraces Mount Vesuvius, without being convinced that it is a fragment of a large volcano, nearly concentric with the present inner cone, which, in some great eruption, had been destroyed all but this fragment. In our own times, an event of no small magnitude has taken place on the same spot; the inner cone of Vesuvius having undergone so great a change during the eruption in 1794, that it now bears no resemblance to what it was when I saw it in 1785.

The general or partial stagnation of the internal lavas at the close of each eruption seems, then, to render it necessary, that in every new discharge the lava should begin by making a violent laceration. And this is probably the cause of those tremendous earthquakes which precede all great eruptions, and which cease as soon as the lava has found a vent. It seems but reasonable to ascribe like effects to like causes, and to believe that the earthquakes which frequently desolate countries not externally volcanic, likewise indicate the protrusion from below of matter in liquid fusion penetrating the mass of rock.

The injection of a whinstone-dike into a frail mass of shale and sandstone, must have produced the same effects upon it that the lava has just been stated to produce on the loose beds of volcanic scoria. One stream of liquid whin, having flowed into such an assemblage, must have given it great additional weight and strength; so that a second stream, coming after the first, would be opposed by a mass the laceration of which would produce an earthquake if it were overcome,

come, or by which, if it resisted, the liquid matter would be compelled to penetrate some weaker mass, perhaps at a great distance from the first. The internal fire, being thus compelled perpetually to change the scene of its action, its influence might be carried to an indefinite extent; so that the intermittence in point of time, as well as the versatility in point of place, already remarked as common to the Huttonian and volcanic fires, are accounted for on our principles: and it thus appears that whinstone possesses all the properties which we are led by theory to ascribe to an internal lava.

This connection is curiously illustrated by an intermediate case between the results of external and internal fire, displayed in an actual section of the antient part of Vesuvius, which occurs in the mountain of Somma, mentioned above. I formerly described this scene in my paper on whinstone and lava; and I must beg leave once more to press it upon the notice of the public, as affording to future travellers a most interesting field of geological inquiry.

The section is seen in the bare vertical cliff several hundred feet in height, which Somma presents to the view from the little valley, in form of a crescent, which lies between Somma and the interior cone of Vesuvius, called the *Atrio del Cavallo*. (Fig. 42. represents this scene, done from the recollection of what I saw in 1785. *abc* is the interior cone of Vesuvius; *dfg* the mountain of Somma; and *cde* the Atrio del Cavallo). By means of this cliff (*fd* in fig. 42, and which is represented separately in fig. 44), we see the internal structure of the mountain, composed of thick beds (*kk*) of loose scoria which have fallen in showers, between which thin but firm streams (*mm*) of lava are interposed which have flowed down the outward conical sides of the mountain. (Fig. 43. is an ideal section of Vesuvius and Somma through the axis of the cones, showing the manner in which the beds of scoria and of lava lie upon each other, the extremities of which beds are seen edgewise in the cliff at *mm* and *kk*, fig. 42, 43, and 44).

This assemblage of scoria and lava is traversed abruptly and vertically by streams of solid lava (*nn*, fig. 44), reaching

8 from

from top to bottom of the cliff. These last I conceive to have flowed in rents of the antient mountain, which rents had acted as pipes through which the lavas of the lateral eruptions were conveyed to the open air. This scene presents to the view of an attentive observer a real specimen of those internal streams which we have just been considering in speculation, and they may exhibit circumstances decisive of the opinions here advanced. For, if one of these streams had formerly been connected with a lateral eruption discharged at more than 600 feet above the Atrio del Cavallo, it might possibly contain the carbonate of lime. But could we suppose that depth to extend to 1708 feet, the interference of air-bubbles, and the action of a stronger heat than was merely required for the fusion of the carbonate, might have been overcome.

Perhaps the height of Vesuvius has never been great enough for this purpose. But could we suppose Ætna to be cleft in two, and its structure displayed, as that of Vesuvius has just been described, there can be no doubt that internal streams of lava would be laid open, in which the pressure must have far exceeded the force required to constrain the carbonic acid of limestone, since that mountain occasionally delivers lavas from its summit, placed 10·954 feet above the level of the Mediterranean *, which washes its base. I recollect having seen, in some parts of Ætna, vast chasms and crags, formed by volcanic revolutions, in which vertical streams of lava, similar to those of Somma, were apparent. But my attention not having been turned to that object till many years afterwards, I have only now to recommend the investigation of this interesting point to future travellers.

What has been said of the heat conveyed by internal volcanic streams, applies equally to that deeper and more general heat by which the lavas themselves are melted and propelled upwards. That they have been really so propelled, from a great internal mass of matter, in liquid fusion, seems to admit of no doubt, to whatever cause we ascribe the heat

* Philosophical Transactions 1777, p. 595.

of

of volcanoes. It is no less obvious, that the temperature of that liquid must be of far greater intensity than the lavas, flowing from it, can retain when they reach the surface. Independently of any actual eruption, the body of heat contained in this vast mass of liquid must diffuse itself through the surrounding substances, the intensity of the heat being diminished by slow gradations, in proportion to the distance to which it penetrates. When, by means of this progressive diffusion, the heat has reached an assemblage of loose marine deposites, subject to the pressure of a great superincumbent weight, the whole must be agglutinated into a mass, the solidity of which will vary with the chemical composition of the substance, and with the degree of heat to which each particular spot has thus been exposed. At the same time, analogy leads us to suppose that this deep and extensive heat must be subject to vicissitudes and intermissions like the external phænomena of volcanoes. We have endeavoured to explain some of these irregularities, and a similar reasoning may be extended to the present case. Having shown that small internal streams of lava tend successively to pervade every weak part of a volcanic mountain, we are led to conceive that the great masses of heated matter just mentioned will be successively directed to different parts of the earth; so that every loose assemblage of matter lying in a submarine and subterranean situation, will, in its turn, be affected by the indurating cause, and the influence of internal volcanic heat will thus be circumscribed within no limits but those of the globe itself.

A series of undoubted facts prove that all our strata once lay in a situation similar in all respects to that in which the marine deposites just mentioned have been supposed to lie.

The inhabitant of an unbroken plain, or of a country formed of horizontal strata, whose observations have been confined to his native spot, can form no idea of those truths which at every step, in an alpine district, force themselves on the mind of a geological observer. Unfortunately for the progress of geology, both London and Paris are placed in countries of little interest; and those scenes by which the principles of this science are brought into view in the most striking

striking manner, are unknown to many persons best capable of appreciating their value. The most important, and at the same time the most astonishing truth which we learn by any geological observations, is, that rocks and mountains now placed at an elevation of more than two miles above the level of the sea, must at one period have lain at its bottom. This is undoubtedly true of those strata of limestone which contain shells; and the same conclusion must be extended to the circumjacent strata. The imagination struggles against the admission of so violent a position, but must yield to the force of unquestionable evidence; and it is proved by the example of the most eminent and cautious observers, that the conclusion is inevitable *.

Another question here occurs, which has been well treated by Mr. Playfair. Has the sea retreated from the mountains? or have they risen out of the sea? He has shown that the balance of probability is incomparably in favour of the latter supposition; since, in order to maintain the former, we must dispose of an enormous mass of sea, whose depth is several miles, and whose base is greater than the surface of the whole sea. Whereas the elevation of a continent out of a sea like ours, would not change its level above a few feet; and even were a great derangement thus occasioned, the water would easily find its level without the assistance of any extraordinary supposition. The elevation of the land, too, is evinced by what has occasionally happened in volcanic regions, and affords a complete solution of the contortion and erection of strata, which are almost universally admitted to have once lain in a plane and horizontal position.

Whatever opinion be adopted as to the mode in which the land and the water have been separated, no one doubts of the antient submarine situation of the strata.

An important series of facts proves that they were likewise subterranean. Every thing indicates that a great quantity of matter has been removed from what now constitutes the surface of our globe, and enormous deposites of loose

* Saussure, *Voyages dans les Alpes*, tom. ii. p. 99—104.

fragments,

fragments, evidently detached from masses similar to our common rock, evince the action of some very powerful agent of destruction. Analogy, too, leads us to believe that all the primary rocks have once been covered with secondary; yet in vast districts no secondary rock appears. In short, geologists seem to agree in admitting the general position, that very great changes of this kind have taken place in the solid surface of the globe, however much they may differ as to their amount, and as to their causes.

Dr. Hutton ascribed these changes to the action, during very long time, of those agents, which at this day continue slowly to corrode the surface of the earth; frosts, rains, the ordinary floods of rivers, &c., which he conceives to have acted always with the same force, and no more. But to this opinion I could never subscribe, having early adopted that of Saussure, in which he is joined by many of the continental geologists. My conviction was founded upon the inspection of those facts in the neighbourhood of Geneva, which he has adduced in support of his opinion. I was then convinced, and I still believe, that vast torrents, of depth sufficient to overtop our mountains, have swept along the surface of the earth, excavating valleys, undermining mountains, and carrying away whatever was unable to resist such powerful corrosion. If such agents have been at work in the Alps, it is difficult to conceive that our countries should have been spared. I made it therefore my business to search for traces of similar operations here. I was not long in discovering such in great abundance; and, with the help of several of my friends, I have traced the indications of vast torrents in this neighbourhood as obvious as those I formerly saw on Saleve and Jura. Since I announced my opinion on this subject, in a note subjoined to my paper on whinstone and lava, published in the fifth volume of the Transactions of this Society, I have met with many confirmations of these views. The most important of these are derived from the testimony of my friend lord Selkirk, who has lately met with a series of similar facts in North America.

It would be difficult to compute the effects of such an agent; but if, by means of it, or of any other cause, the whole

whole mass of secondary strata, in great tracts of country, has been removed from above the primary, the weight of that mass alone must have been sufficient to fulfil all the conditions of the Huttonian theory, without having recourse to the pressure of the sea. But when the two pressures were combined, how great must have been their united strength!

We are authorised to suppose that the materials of our strata, in this situation, underwent the action of fire. For volcanoes have burnt long before the earliest times recorded in history, as appears by the magnitude of some volcanic mountains; and it can scarcely be doubted that their fire has acted, without any material cessation, ever since the surface of our globe acquired its present form. In extending that same influence to periods of still higher antiquity, when our strata lay at the bottom of the sea, we do no more than ascribe permanence to the existing laws of nature.

The combination of heat and compression resulting from these circumstances, carries us to the full extent of the Huttonian theory, and enables us, upon its principles, to account for the igneous formation of all rocks from loose marine deposites.

The sand would thus be changed to sandstone, the shells to limestone, and the animal and vegetable substances to coal.

Other beds, consisting of a mixture of various substances, would be still more affected by the same heat. Such as contained iron, carbonate of lime, and alkali, together with a mixture of various earths, would enter into thin fusion, and, penetrating through every crevice that occurred, would, in some cases, reach what was then the surface of the earth, and constitute lava: in other cases it would congeal in the internal rents, and constitute porphyry, basalt, greenstone, or any other of that numerous class of substances which we comprehend under the name of *whinstone.* At the same time, beds of similar quality, but of composition somewhat less fusible, would enter into a state of viscidity, such as many bodies pass through in their progress towards fusion. In this state, the particles, though far from possessing the same freedom as in a liquid, are susceptible of crystalline arrangement;

arrangement*; and the substance, which, in this sluggish state, would be little disposed to move, being confined in its original situation by contiguous beds of more refractory matter, would crystallize without undergoing any change of place, and constitute one of those beds of whinstone which frequently occur interstratified with sandstone and limestone.

In other cases, where the heat was more intense, the beds of sand, approaching more nearly to a state of fusion, would acquire such tenacity and toughness, as to allow themselves to be bent and contorted, without laceration or fracture, by the influence of local motions, and might assume the shape and character of primary schistus; the limestone would be highly crystallized, and would become marble, or, entering into thin fusion, would penetrate the minutest rents in the form of calcareous spar. Lastly, when the heat was higher still, the sand itself would be entirely melted, and might be converted, by the subsequent effects of slow cooling, into granite, sienite, &c.; in some cases, retaining traces of its original stratification, and constituting gneiss and stratified granite; in others, flowing into the crevices, and forming veins of perfect granite.

In consequence of the action of heat upon so great a quantity of matter, thus brought into a fluid or semifluid state, and in which, notwithstanding the great pressure, some substances would be volatilized, a powerful heaving of the superincumbent mass must have taken place, which, by re-

* This state of viscidity, with its numberless modifications, is deserving of great attention, since it affords a solution of some of the most important geological questions. The mechanical power exerted by some substances in the act of assuming a crystalline form, is well known. I have seen a set of large and broad crystals of ice, like the blade of a knife, formed in a mass of clay of such stiffness that it had just been used to make cups for chemical purposes. In many of my former experiments I found that a fragment of glass, made from whinstone or lava, when placed in a muffle heated to the melting point of silver, assumed a crystalline arrangement, and underwent a complete change of character. During this change it became soft, so as to yield to the touch of an iron rod; yet retained such stiffness, that, lying untouched in the muffle, it preserved its shape entirely, the sharp angles of its fracture not being in the least blunted.

peated

peated efforts, succeeding each other from below, would at last elevate the strata into their present situation.

The Huttonian theory embraces so wide a field, and comprehends the laws of so many powerful agents exerting their influence in circumstances and in combinations hitherto untried, that many of its branches must still remain in an unfinished state, and may long be exposed to partial and plausible objections, after we are satisfied with regard to its fundamental doctrines. In the mean time I trust that the object of our pursuit has been accomplished, in a satisfactory manner, by the fusion of limestone under pressure. This single result affords, I conceive, a strong presumption in favour of the solution which Dr. Hutton has advanced of all the geological phænomena; for the truth of the most doubtful principle which he has assumed, has thus been established by direct experiment.

APPENDIX.

No. I.

Specific Gravity of some of the foregoing Results.

As many of the artificial limestones and marbles produced in these experiments were possessed of great hardness and compactness, and as they had visibly undergone a great diminution of bulk, and felt heavy in the hand, it seemed to me an object of some consequence to ascertain their specific gravity, compared with each other, and with the original substances from which they were formed. As the original was commonly a mass of chalk in the lump, which, on being plunged into water, begins to absorb it rapidly, and continues to do so during a long time, so as to vary the weight at every instant, it was impossible, till the absorption was complete, to obtain any certain result; and to allow for the weight thus gained, required the application of a method different from that usually employed in estimating specific gravity.

 In

In the common method, the substance is first weighed in air, and then in water; the difference indicating the weight of water displaced, and being considered as that of a quantity of water equal in bulk to the solid body. But as chalk, when saturated with water, is heavier, by about one-fourth, than when dry, it is evident that its apparent weight in water must be increased, and the apparent loss of weight diminished exactly to that amount. To have a just estimate, then, of the quantity of water displaced by the solid body, the apparent loss of weight must be increased by adding the absorption to it.

Two distinct methods of taking specific gravity thus present themselves, which it is of importance to keep separate, as each of them is applicable to a particular class of subjects.

One of these methods consists in comparing a cubic inch of a substance in its dry state, allowing its pores to have their share in constituting its bulk, with a cubic inch of water.

The other depends upon comparing a cubic inch of the solid matter of which the substance is composed, independently of vacuities, and supposing the whole reduced to perfect solidity, with a cubic inch of water.

Thus, were an architect to compute the efficacy of a given bulk of earth, intended to load an abutment, which earth was dry, and should always remain so, he would undoubtedly follow the first of these modes: whereas, were a farmer to compare the specific gravity of the same earth with that of any other soil, in an agricultural point of view, he would use the second mode, which is involved in that laid down by Mr. Davy.

As our object is to compare the specific density of these results, and to ascertain to what amount the particles have approached each other, it seems quite evident that the first mode is suited to our purpose. This will appear most distinctly by inspection of the following table, which has been constructed so as to include both.

Table

Table of Specific Gravities.

I.	II. Weight in Air, dry.	III. Weight in Water.	IV. Weight in Air, wet.	V. Difference between Columns II. & III.	VI. Difference between Columns II. & IV., or Absorption.	VII. Absorption per cent.	VIII. Sum of Columns V. & VI.	IX. Specific Gravity by common Mode.	X. Specific Gravity by new Mode.
1.	125·90	77·55	135·65	47·35	9·75	7·74	57·10	2·604	2·204
2.	9·94	6·13	9·99	3·81	0·05	0·50	3·86	2·602	2·575
3.	15·98	9·70	16·02	6·28	0·04	0·25	6·32	2·544	2·528
4.	5·47	3·33	5·48	2·14	0·01	0·18	2·15	2·556	2·544
5.	18·04	10·14	18·06	7·90	0·02	0·11	7·92	2·283	2·277
6.	6·48	3·74	7·10	2·74	0·62	9·56	3·36	2·365	1·928
7.	10·32	5·97	10·36	4·35	0·04	0·39	4·39	2·372	2·350
8.	54·57	31·30	55·23	23·27	0·66	1·21	23·93	2·345	2·280
9.	72·27	41·10	76·13	31·17	3·86	5·34	35·03	2·318	2·063
10.	37·75	21·15	38·30	16·60	0·55	1·45	17·15	2·274	2·291
11.	21·21	12·55	21·26	8·66	0·05	0·24	8·71	2·449	2·435
12. Marble.	18·59	11·56	18·61	7·03	0·02	0·18	7·05	2·644	2·636
13. Chalk.	504·15	302·40	623·20	201·75	119·05	23·61	320·80	2·498	1·571
14. Average Chalk.	444·30	264·35	550·80	179·95	106·50	23·97	286·45	2·469	1·551
15. Rammed Powder.	283·97	—	—	—	—	—	198·65	—	1·429

Explanation.

Column I. contains the number affixed to each of the specimens whose properties are expressed in the table.

The first eleven are the same with those used in the paper read in this society on the 30th of August 1804, and published in Nicholson's Journal for October following, and which refer to the same specimens. No. 12. is a specimen of yellow marble, bearing a strong resemblance to No. 3. No. 13. a specimen of chalk. No. 14. shows the average of three trials with chalk. No. 15. some pounded chalk, rammed in the manner followed in these experiments. In order to ascertain its specific gravity, I rammed the powder into a glass tube, previously weighed; then, after weighing the whole, I removed the chalk, and filled the same tube

with

with water. I thus ascertained, in a direct manner, the weight of the substance, as stated in Column II., and that of an equal bulk of water, stated in Column VIII.

Column II. Weight of the substance, dry in air, after exposure, during several hours, to a heat of 212° of Fahrenheit.

Column III. Its weight in water, after lying long in the liquid, so as to perform its full absorption; and all air-bubbles being carefully removed.

Column IV. Weight in air, wet. The loose external moisture being removed by the touch of a dry cloth, but no time being allowed for evaporation.

Column V. Difference between Columns II. and III., or apparent weight of water displaced.

Column VI. Difference between Columns II. and IV., or the absorption.

Column VII. Absorption reduced to a per centage of the dry substance.

Column VIII. Sum of Columns V. and VI., or the real weight of water displaced by the body.

Column IX. Specific gravity, by the common mode, resulting from the division of Column II. by Column V.

Column X. Specific gravity, in the new mode, resulting from the division of Column II. by Column VIII.

The specific gravities ascertained by the new mode, and expressed in Column X. correspond very well to the idea which is formed of their comparative densities, from other circumstances, their hardness, compact appearance, susceptibility of polish, and weight in the hand.

The case is widely different when we attend to the results of the common method contained in Column IX. Here the specific gravity of chalk is rated at 2·498, which exceeds considerably that of a majority of the results tried. Thus, it would appear, by this method, that chalk has become lighter by the experiment, in defiance of our senses, which evince an increase of density.

This singular result arises, I conceive, from this; that, in our specimens, the faculty of absorption has been much more decreased than the porosity. Thus, if a piece of crude chalk,

chalk, whose specific gravity had previously been ascertained by the common mode, and then well dried in a heat of 212°, were dipped in varnish, which would penetrate a little way into its surface; and, the varnish having hardened, the chalk were weighed in water, it is evident that the apparent loss of weight would now be greater by 23·61 per cent. of the dry weight than it had been when the unvarnished chalk was weighed in water; because the varnish, closing the superficial pores, would quite prevent the absorption, while it added but little to the weight of the mass, and made no change on the bulk. In computing, then, the specific gravity by means of this last result, the chalk would appear very much lighter than at first, though its density had, in fact, been increased by means of the varnish.

A similar effect seems to have been produced in some of these results by the agglutination or partial fusion of part of the substance, by which some of the pores have been shut out from the water.

This view derives some confirmation from an inspection of Columns VI. and VII.; the first of which expresses the absorption; and the second, that result reduced to a per centage of the original weight. It there appears, that whereas chalk absorbs 23·97 per cent., some of our results absorb only 0·5, or so low as 0·11 per cent. So that the power of absorption has been reduced from about one-fourth to less than the five hundredth of the weight.

I have measured the diminution of bulk in many cases, particularly in that of No. 11. The chalk, when crude, ran to the 75th degree of Wedgewood's gage, and shrunk so much during the experiment, that it ran to the 161st.; the difference amounting to 86 degrees. Now, I find that Wedgewood's gage tapers in breadth from 0·5 at zero of the scale, to 0·3 at the 240th degree. Hence we have for one degree 0·000833. Consequently, the width, at the 75th degree, amounts to 0·437525; and at the 161st, to 0·365887. These numbers, denoting the linear measure of the crude chalk, and of its result under heat and compression, are as 100 to 83·8; or, in solid bulk, as 100 to 57·5. Computing the densities from this source, they are as 1 to 1·73. The spe-

cific gravities in the table, of the chalk, and of this result, are as 1·551 : 2·435; that is, as 1 to 1·57. These conclusions do not correspond very exactly; but the chalk employed in this experiment was not one of those employed in determining average specific gravity in the table; and other circumstances may have contributed to produce irregularity. Comparing this chalk with result second, we have 1·551 : 2·575, so 1 : 1·6602.

No. II.

Table containing the Reduction of the Forces mentioned in Chap. VII. to a common Standard.

I. Number of Experiment referred to in Chap VII.	II. Bore, in Decimal of an Inch.	III. Pressure in Hundred Weights	IV. Temperature by Wedgewood's Pyrom.	V. Depth of Sea in Feet.	VI. Ditto in Miles.	VII. Pressure, expressed in Atmospheres.
1	0·75	3	22	1708·05	0·3235	51·87
2	0·75	3	25	1708 05	0·3235	51·87
3	0·75	10	20	5693·52	1·0783	172·92
4	0·75	10	31	5693·52	1·0783	172·92
5	0·75	10	41	5693·52	1·0783	172·92
6	0·75	10	51	5693·52	1·0783	172·92
7	0·75	10	—	5693·52	1·0783	172·92
8	0·54	2	—	2196·57	0·4160	66·71
9	0·54	4	—	4393·14	0·8320	133·43
		8·1	—	8896·12	1·6848	270·19
10	0·75	3	21	1708·05	0·3235	51·87
11	0·75	4	25	2277·41	0·4313	69·70
12	0·75	5	—	2846·76	0·5396	86·46

Explanation.

Column I. contains the number of the experiment, as referred to in the text. Column II. The bore of the barrel used, in decimals of an inch. Column III. The absolute force applied to the barrel, in hundred weights. Column IV. The temperature in Wedgewood's scale. Column V. The depth of sea at which a force of compression would be exerted equal to that sustained by the carbonate in each experiment, expressed in feet. Column VI. The same in miles.

miles. Column VII. Compressing force, expressed in atmospheres.

Both tables were computed separately, by a friend, Mr. J. Jardine, and myself.

The following data were employed:

Area of a circle of which the diameter is unity, 0·785398.

Weight of a cubic foot of distilled water, according to professor Robison, 998·74 ounces avoirdupois.

Mean specific gravity of sea water, according to Bladh, 1·0272.

Mean height of the barometer at the level of the sea 29·91196 English inches, according to Laplace.

Specific gravity of mercury, according to Cavendish and Brisson, 13·568.

XXXIII. *Catalogue of Specimens, showing the Result of Sir* James Hall's *Experiments on the Effects of Heat modified by Compression; which were deposited by Sir* James Hall *in the British Museum on the 28th of June* 1806.

Numbers 1, 2, 3, 4, 5, 6, and 7, were all produced, in separate experiments, from pounded carbonate of lime. No. 1. was obtained in 1799. It is a firm stone, requiring a smart blow of a hammer to break it. It was enclosed in a cartridge of paper, the mark of which it still bears. The other six are still harder, and more compact, approaching nearly in these qualities to common limestone.

Nos. 2, 4, and 7, possess a degree of semi-transparency most remarkable in No. 4; and all of these specimens exhibit an uneven fracture, approaching to that of bees' wax and marble. Their colours are variously, though slightly, tinged with yellow and blue; in particular No. 3, which, though produced from common white chalk, resembles a yellow marble.

Nos. 3, 5, and 6, have taken a tolerable polish. No. 7. contains a shell introduced along with the pounded chalk, and now closely incorporated with it.

Along with No. 3. is a specimen (A 3.) of common yellow marble, bearing a strong resemblance to the artificial stone.

Nos. 8, 9, 10, 11, all formed from pieces of chalk exposed unbroken to heat and pressure.

No. 8. is remarkable for a shining grain and semi-transparency. Nos. 9 and 10. show parallel planes like internal stratification, which has often appeared in chalk in consequence of the action of heat, though nothing of the kind could be seen in the native mass. No. 11. very compact, and of a yellow colour. Nos. 12 and 13. examples of welding, in which the pounded chalk has been incorporated with a lump of chalk upon which it had been rammed, so that their joining is hardly visible in the fracture.

Nos. 14, 15, 16, showing the fusion of the carbonate well advanced, with a considerable action on the porcelain tube.

In No. 15. the rod of chalk is half melted, and a yellow substance produced by a mixture of the carbonate with the porcelain. No. 16. is a lump of chalk in a state indicating softness, a piece of porcelain which lay in contact with it having sunk a little into the substance of the carbonate.

Nos. 17 and 18, and all the following numbers, being delicate, are inclosed in tubes of glass, and fixed with sealing-wax on little cups of wood.

No. 17, formed from pounded chalk, shows in one part the most complete formation of spar, with its rhomboidal fracture, I ever obtained.

The carbonate, having lost some of its carbonic acid, had crumbled so much in its essential parts by the action of the air, that the crystallization was no longer visible; and I had given up the specimen for lost, till some time in July 1804, when employed in examining these results, in order to show them in the Royal Society of Edinburgh, a mass of the carbonate broke in two and exhibited the fracture now before us, nearly in as good a state as it was originally. I immediately inclosed it in a glass tube, and sealed it with wax, so that I have hopes of preserving it; and it still continues entire, though now sealed up for a year and a half.

 No. 18,

No. 18, likewise from pounded chalk, is perfectly fresh and entire, though made more than two years ago; it shows some beautiful clear crystals of spar in parallel plates, but they are so small as to require the use of a glass.

Nos. 19, 20, 21, show examples of fusion and action on the tubes. In No. 19. a shell is finely united to some pounded chalk. In No. 20. the mass, originally of pounded chalk, is sinking upon itself, and acting at the same time upon the tube; the fracture of the carbonate, in its pure parts, showing brilliant facettes of crystallization. In No. 21, the carbonate in a state like the last, the compound of porcelain and carbonate showing its liquidity by penetrating the tube so as to form a distinct vein of a dark colour, and then spreading on its outside to a considerable extent, terminating with the black line alluded to in the account of the experiments.

Nos. 22, 23, 24, give proofs of entire fusion. In No. 22. we see two porcelain tubes inclosed for preservation in a glass tube: the end attached to the little wooden cup must be held downwards, to show the position in which the experiment was made. The innermost porcelain tube stands with its muzzle upwards, and the outermost covers it in the inverse position. The carbonate was contained in the inner tube: during the action of heat, the barrel failed suddenly, and the carbonate has boiled over the lips of the inner tube, running down, as here appears, almost to its bottom; thus proving that immediately previous to the failure of the apparatus the carbonate had been in a liquid state.

No. 23, two masses of carbonate welded together in a complete state of froth. The substance shining and semi-transparent. No. 24, two separate masses exposed together to heat; one from pounded chalk, now in a state quite like the last; the other put in as a lump of chalk, dressed flat at both ends, and a letter cut on each end (as done in many of the experiments). It is in a shining and almost transparent state; at one end the flat form and the letter are visible; the other end is completely rounded in fusion, with a glassy surface.

No. 25.

No. 25. shows the substance produced by the combination of carbonate of lime with pure silex. Part of the porcelain tube in this specimen is filled with pounded silex, which, having a very feeble agglutination, is supported by some sealing-wax. Upon the silex during the experiment had lain some carbonate of lime, the lower part of which had united with the silex, producing a semi-transparent substance with a delicate tinge of blue. The termination of this compound, as it had advanced downwards into the silex, shows the round and mammillated form of chalcedony.

No. 26, result of an experiment with heat and compression, made July 22, 1805, with some pure carbonate of lime prepared by Mr. Hatchett. The carbonate was inclosed in a small tube of platina, and was thus secured against all contamination.

No. 27, result of an experiment made likewise in platina with a fragment of a periwinkle shell: the form of the shell is still visible, though the substance is glazed by semi-fusion. Along with, on the same stand, is a small drop like a pearl, formed by the entire fusion of one of the fragments; and a portion of shell of the same kind, in its natural state, is introduced, in order to show what change had taken place during the experiment.

No. 28. is a specimen of coal produced from horn; it is a shining black substance, exactly resembling pitch, or black sealing-wax: it was formed in a low red heat, and in circumstances of compression; by which, while some of the volatile parts of the original were allowed to separate, others were retained. It has thus acquired a jet black colour, while it retains its inflammability, and burns with bright flame.

No. 29. likewise produced from an animal substance, flannel. In this case, none of the component parts of the original substance seem to have separated from it, owing either to less heat or greater closeness than in the last case. The consequence has been, that the original colour has undergone much less change, being of a yellow red, at the same time the substance has been in a state of fusion, and has assumed

assumed a polish by moulding itself on the glass into which it had been pressed. This result seems to bear some analogy to the substance called *retinasphaltum* described by Mr. Hatchett.

No. 30. is a piece of wood partially converted into coal by heat and compression. In some parts the substance entirely resembles pitch, being full of large and shining air-holes; in others, the fibres of the wood are still distinctly visible. The whole is jet black, and burns with a bright flame.

No. 21. is a specimen of the substance, like wool, formed in several of these experiments by the exudation of the fusible metal through the barrels of iron; the metal, in a liquid state spouting to a considerable distance, and depositing this substance upon any obstacle opposed to the stream.

XXXIV. *Memoir upon the Germination and Fermentation of Grains and Farinaceous Substances. By Messrs.* VAUQUELIN *and* FOURCROY.

[Continued from p. 182.]

§ IV. *Analysis of Garden Beans.*

THE infusion of the farina of garden beans, drawn clear off, and put into a phial well corked and completely filled, becomes troubled like milk, and makes an abundant deposit, which clears it up. Left in the phial for twenty days, it liberates no gas; it is acid, preserves the taste of beans, reddens turnsole, and is precipitated, by means of lime water, in transparent flakes: by the oxalate of ammonia it is precipitated abundantly; by ammonia, but slightly; by gall-nuts, in flakes like wine lees; by the nitrates of mercury and silver, in yellowish white; and by the prussiate of potash, in green flakes which become blue.

The spontaneous deposit becomes transparent upon drying, and burns like horn.

The same infusion, put into a large bottle about three-fourths empty, acts in the same manner as at first; it diminishes

minishes the volume of air, which afterwards contains a fifth part of carbonic acid, and the residue of which is then formed of 97·5 of azotic gas and of 2·5 of oxygen gas. The liquor assumes a smell slightly putrid, without acidity; it precipitates lime water, gall-nuts, &c. &c.

The precipitate formed by lime water is of a purple colour, which blackens on drying; it yields ammonia on being burnt, and leaves a gray cinder, soluble in the muriatic acid with effervescence, from which ammonia precipitates it in gelatinous flakes, and the prussiate of potash in white. Thus this precipitate contains an animal matter—phosphate of lime and phosphate of iron, besides the phosphate of potash remarked by M. Saussure junior.

We burned some dry garden beans to ashes in a platina crucible. The taste of these ashes was alkaline and caustic; they contained potash and phosphates of lime, magnesia, and iron, which the nitric acid dissolves.

Garden beans also contain starch, an animal matter, phosphates of lime, magnesia, potash, iron, and free potash. No sugar is found in it, at least not in any appretiable quantity.

The tunic, or coat of these beans, contains tannin in abundance.

This analysis explains, 1st, why beans putrefy so easily, and become infectious: 2dly, why they are so nourishing, and susceptible of filling the place of all other food: 3dly, why, when cooked with the skins on, they are better preserved: and, 4thly, why this article yields at once the aliment, the basis, and the materials proper to form and colour the blood, and to nourish the bones.

§ V. *Analysis of Lentils.*

The farina of lentils, macerated in water, spreads the smell of this green leguminous herb; after an hour's maceration the water does not become clear, except after two filtrations; its taste is weak and nauseous; it is not acid; it precipitates abundantly by gall-nuts and the oxymuriatic acid, as well as by sulphate of iron: lime water renders it milky. It becomes troubled spontaneously, and becomes speedily milky;

milky; the alkalis render it clear by making it yellow; the acids, which clear it at first, put into it afterwards in excess, precipitate it strongly. This seems to indicate that the liquor owes its limpidity to the alkali which absorbs the acid, either spontaneous or added.

The infusion grows frothy and coagulates at a boiling heat; when filtered, it precipitates, but less abundantly, by the re-agents indicated.

Upon inclosing, at a low temperature, this infusion, already troubled, in two flasks, the one filled and the other three-fourths empty, with Woolf's tubes, we saw it become clear, and deposit white flakes after some hours. We did not observe, after a few days, any apparent change of the liquor, disengagement of gas, or absorption of air; the water was acescent, and the air above it contained a little carbonic acid gas.

Lime hindered the spontaneous precipitation of the infusion until it was saturated by its fermented acid.

Fifteen parts of alcohol, digested several times upon the coarser farina of lentils, was coloured a greenish yellow, and acquired a bitter and sharp taste. When distilled, the produce yielded a very strong smell of *vanilla*, which water caused to disappear by changing it into another very disagreeable. The residue of this distillation is yellowish green; a thick green oil floats at the surface; the liquor is thick and gluey, of a saponaceous smell, and of a rancid taste; the acids and lime water coagulate it like a water of soap. Sulphuric acid, by decomposing it, collects on its surface a rancid greenish oil of the smell of *populeum:* upon evaporating the water a black residue is obtained of a saline appearance, but in such a small quantity that we could not determine the nature of it.

Whole lentils, macerated in double their weight of water, yield, after twenty-four hours, a greenish yellow infusion of an astringent taste. Water precipitates a strong size, sulphate of iron a fine blue, and acetate of lead a yellowish white, without reddening the blue colours: lentils, stripped of their skin, afford no traces of tannin, which seem only to belong to this envelope.

When

When cleaned completely by means of water, the skins of these lentils macerated in alcohol gave it a fine yellowish green colour; spontaneously evaporated, this alcohol deposited green flakes and a crust of the same colour; it blackened the solution of iron. After this double treatment, these skins are dry and arid. They furnished upon distillation a good deal of oil, the smell and taste of which resembled tobacco smoke; the water of the distillation is acid, but yields ammonia by means of potash. Thus, besides their feculent matter, lentils contain a kind of albumen and a little green oil: their bark contains tannin and more oil.

§ VI. *Analysis of the Farina of Lupines.*

1st, This farina is yellow and very bitter; when exposed to the fire, it exhales an animal odour.

2d, Distilled in a retort it yields three-twelfths of charcoal, nearly seven-twelfths of a fœtid red oil, a twelfth of phlegm, and a twelfth of crystallized carbonate of ammonia. The water contains a little ammoniacal acetate. We find in the charcoal phosphate of potash, because its watery ley precipitates calcareous phosphate by the addition of lime water. We also find in the ashes of the burned farina of lupines, phosphates of lime, magnesia, and iron.

3d, It colours alcohol yellow, and renders it bitter; the latter, spontaneously evaporated, leaves a yellow, thick, and very bitter oil, forming a-seventh of the weight of the farina, which almost entirely dissipates upon burning coals, with the smell of fat oil.

4th, The farina of lupines gives water a yellow colour, a bitter taste, and a frothy quality, without rendering it either acid or alkaline. This water precipitates white flakes by the oxymuriatic acid; a purple coagulum by infusion of galls*; very abundant white flakes by the nitrates of mercury and silver, and the acetate of lead: there are a few muriates, not soluble by the nitric acid, in the latter precipitates. It yields also yellowish flakes by means of lime water, and a white powder of calcareous oxalate by means of the ammoniacal oxalate.

* M. Vauquelin attributes this colour to a little phosphate of iron.

5th,

5th, The farina, treated twice successively by alcohol and water, is dissolved almost entirely afterwards in the concentrated acetic acid: this solution, by means of the infusion of galls, precipitates in abundance oxymuriatic acid, ammonia, and mercury.

6th, Diluted in water, and exposed to a gentle heat, the farina of lupine ferments, exhales carbonic acid, forms acetic acid, without any vestige of alcohol, and soon putrefies, exhaling a fœtid odour.

It results from this analysis that the farina of lupines contains:

1st, A bitter and coloured oil, to the amount of a seventh, which communicates its properties to the whole mass.

2d, A vegeto-animal matter, soluble in plenty of water, and much more so in the acetic acid. It is this which furnishes oil and ammonia upon distillation, and which gives to the watery infusion all its properties of precipitation.

3d, Phosphates of lime and magnesia abundant enough, and small quantities of phosphates of potash and iron.

4th, It contains neither starch nor sugar, and, on this account, differs from the other leguminous farinas.

§ VII. *Upon the Germination of Leguminous Seeds.*

1st, In Floreal of the year 12, we placed lentils and garden beans, freed from their husks, under a bell-glass full of atmospheric air, placed upon water, and in a capsule of porcelain. The former germinated three or four days afterwards; their radicles were very long, and the plumules very perceptible; twelve days afterwards their height was three centimetres; their leaves were displayed. The beans had no sign of germination at all; their radicles, however, were lengthened, without the plumule having made any progress. They began to turn mouldy, and at this period the experiment was stopped. The air of the bell-glass extinguished a taper and precipitated lime water, although it still allowed phosphorus to burn a little.

2d, The same seeds, placed at the same period under a bell-glass full of hydrogen gas placed upon water, presented no appearance of germination, not even a development of the

the radicle; they preserved their freshness and consistence; the beans did not turn mouldy. The hydrogen gas contained carbonic acid entirely formed by the seeds, which afterwards germinated in the open air. Thus hydrogen gas does not favour germination; but it does not take away from seeds the property of germinating.

3d, Garden beans macerated in water, deprived of their skin, and afterwards replunged in this liquid, did not germinate in the course of eight days; the water became sour, and assumed the smell of sour cheese.

4th, The water sharpened with a little oxymuriatic acid had no more success. The lentils, treated in the same manner, did not germinate. These seeds no longer germinated in the air; those which had been plunged in water, on the contrary, germinated in the air.

5th, The same seeds peeled, covered with a little water, enough, however, to be deprived of the contact of the air, putrefied instead of germinating. When only moistened, however, and without being deprived of this contact, they germinated very well and sent out coloured leaves, although in the shade.

We may conclude from these experiments that the influence of the air is requisite for germination, as has been already announced by M. de Saussure.

§ VIII. *Experiments upon the Fermentation of Grains.*

1st, Two pounds of ground germinated barley, placed with six pounds of water heated to 55° (131° Fahr.) in a matrass furnished with a crooked tube, fermented in four hours in a heat of 22° (72° Fahr.) The fermentation continued 36 hours. The gas disengaged and collected was partly formed of carbonic acid and partly of hydrogen gas pure enough. Six days afterwards this barley was distilled, from which a product was obtained equal at least to one-third of the water employed. This produce, heavier than water, was acid and empyreumatic. This acidity demonstrates the conversion of alcohol into acetous acid. The liquor, which was saccharine at the period of distillation, was no longer so afterwards.

2d, The

2d, The same ground and germinated barley, but deprived of its bran by bolting, was treated in the same manner as in the first experiment; it fermented with the same appearances, and yielded an equal part of carbonic acid gas and hydrogen gas. Thus the bran was not the source of the latter gas, as at first supposed.

3d, Brewers' mash, exposed in the same apparatus to the same temperature of 22 degrees, fermented more quickly with a more rapid effervescence, and its gas was merely carbonic acid, without hydrogen gas. Thus the latter depends upon the farina mixed with flour.

4th, The farina of germinated barley, with water, exposed in the matrass to a temperature of 15° (59° of F.) did not ferment until the end of five hours; and its gas was condensed by potash. Upon raising the temperature to 22 degrees, there came off a mixture of gas not soluble and inflammable, the proportion of which was soon equal to that of the carbonic acid. Thus it is necessary that there should be a heat of upwards of 20° (68° of F.) before there can be any liberation of hydrogen gas in the farina of barley which is fermenting.

5th, Six pounds of ground barley, not germinated, treated at three several times with twelve pounds of warm alcohol, furnished one ounce two drachms of pure sugar; while six pounds of germinated barley, treated in the same manner, yielded four ounces and two drachms, or about 5 per cent; which is four times what the barley contained previous to germination. Thus, germination forms sugar, as we have announced.

6th, We put 24 pounds of farina of barley, not germinated, into a tub with seven times its weight of hot water at 70° (158° of F.) and four pounds of mild beer yeast. Fermentation immediately commenced with great violence, and continued seven days. The liquor submitted to distillation, with the husks, yielded nine litres of a weak and empyreumatic liquid, which, being passed again through the still, furnished 16 decilitres of an alcohol at 16 degrees, which comes to nine decilitres at 40 degrees. These nine decilitres, weigh-

ing 23 ounces, and 24 pounds of barley not germinated, containing only five ounces of sugar, it follows, that four times more alcohol was formed than there was sugar in this farina. Lavoisier, however, asserts that 100 pounds of sugar furnish only 58 pounds of alcohol.

7th, Twenty-four pounds of germinated and ground barley, made to ferment under the same circumstances as barley not germinated, presented the same phænomena, and only varied in their products. There were two litres 0·3 of alcohol at 40 degrees, which makes five pounds of alcohol for a quintal of barley, or three times more alcohol than there was sugar; and this answers to the produce of barley not germinated.

It must be concluded from these results that it is some other substance than sugar which is converted into alcohol, although sugar is indispensable to its production and to the establishment of fermentation.

8th, Two pounds of farina of bolted wheat, mixed with six pounds of water at 60° (140° F.) remained six hours without motion. The next day, after having remarked the swelling of the mass, we placed the matrass upon a sand-bath a little heated, and added water to favour the disengagement of the gas. We obtained hydrogen gas twice larger in volume than carbonic acid. The vessel, having been taken off the sand-bath, the temperature having decreased to 14° (57° F.) the fermentation all at once stopped. The liquid, when submitted to distillation, did not yield alcohol, but an acid liquor.

The farina of wheat, therefore, does not form alcohol by fermentation: yeast is indispensable for this fermentation, although it does not enter into the composition of alcohol; by accelerating the alcoholic fermentation, it opposes the formation of vinegar. When, on the contrary, the fermentation is very slow, the alcohol becomes acetous in proportion as it is formed; perhaps even then sugar and the other fermenting substances pass into the acid state without alcoholizing.

XXXV. *Ob-*

XXXV. *Observations upon the Disease, in Sheep, called in England Foot-rot.* *By* M. CHARLES PICTET, *of Geneva**.

I THINK I shall render a service to the proprietors of sheep by calling their attention to a malady which, to my knowledge, has not been described by the veterinarists of any country, and which, to the present moment, appears to have been unknown in France †. The following is the occasion upon which I observed it:

In the month of May 1804, I received from Piedmont a flock of 200 sheep of various mongrel breeds of the second and third generations. The animals came to hand in good condition, but some of them were lame. The flock was placed, with a hundred other mongrels, on a flat mountain, the pasturage of which is healthy and of good quality. We did not pay very great attention to the lame sheep, because in general, upon a journey, they cripple often from fatigue alone, and their lameness goes off after resting a while. I never yet received a lot of Spanish sheep among which there was not a few lame ones at their arrival; but this defect was never of long continuance.

In the present case, however, the lame animals became worse and worse, and every day others of them began to grow lame, while none of the others grew any better. Not suspecting any contagion, we attributed this affection to the rocky nature of the pasturage; to the frequent journeys which the sheep took from a rivulet to go and feed; and also to the circumstance that the sheep-cot was not frequently enough renewed. We took precautions against all these various causes, and yet the malady continued among the sheep. At the end of six weeks every one among them was lame; and some of them were affected in all their four legs. They crawled upon their knees while feeding, and the worst of them fell off very much in their appearance. It now

* From *Bib. Brit,* vol. x, p. 371.

† The *Pietin*, or *Pietine*, or the *Fourchet*, which Carlier and others have mentioned, is a slight malady, not contagious, and wholly different from that which I am about to describe.

became indispensably necessary to assist this flock by every means in our power. We removed them to the distance of six leagues; their removal was not effected without great trouble, and was very tedious: we also had recourse to carriages for conveying the most diseased among them; but in spite of all our care, many fell a victim to the disease, unable to bear the fatigue.

The different individuals of the flock presented all the varieties of the disease; which may be reduced to three principal ones. The animals in the first stage of the disease were only a little lame, appeared without fever, and preserved their appetite. Upon inspecting the foot, there was only a slight redness discovered at the root of the hoofs, or a slight oozing out of matter round the hoof; sometimes only a slight degree of heat in the lame foot, without any apparent irritation.

The sheep which had the malady in the second degree were lame all fours, had a fever, appeared dull, fed slowly, and were often on their knees if the fore-legs were attacked. Upon inspecting the foot, there was an ulceration, as well at the root of the division of the hoofs, as at the juncture of the horn to the leg, accompanied by a whitish and fetid sanies.

Such animals as were in the third degree of the disease had a continual fever; they were meagre and sorrowful, rose up with difficulty, and lost their wool. The ulceration of the feet was venomous, and resembled a white gangrene. Purulent collections were formed under the hoof, and made their appearance at the junction of the horn and the skin. Among some sheep the hoof was detached or entirely destroyed, and the flesh of the two divisions of the foot was one complete ulcer. In others the hoof had kept on; because the flowing of the purulent matter made its appearance at the sole, and had gnawed and completely destroyed it. In this case, the interior of the foot, upon turning it up to look at it, offered only a putrid mass filled with worms, contained in the horn of the hoof; the flesh and the ligaments appeared completely destroyed, and the bones of the feet were carious; the smell was cadaverous and insupportable.

We

We endeavoured at first to classify and separate the animals according to the stage of the disease. The antiseptic lotions, such as red wine, vinegar, extract of bark, and oak bark, were employed; as also fumigations of nitric acid, to weaken the putrid tendency, and second the effect of the remedies.

I heard from Piedmont, that the vitriol of copper in powder, as a drying caustic, was very useful at the commencement of the disorder in checking its progress. We employed it, as well as martial vitriol, without any remarkable success, upon such animals as were only slightly attacked. It is probable that the contagious influence, which we had not yet learnt to guard against sufficiently, had destroyed the effect of this remedy.

The acetite of lead or saturnine extract was employed with more advantage. Antimonial beer was useful in drying the wound, and the lapis infernalis in burning the bad flesh, which was speedily reproduced after the incisions which accompanied the complete cleaning of the feet.

The treatment of a numerous flock in this miserable situation is extremely perplexing. Four shepherds and several assistants were employed in taking care of the 300 lame sheep, and it was an extremely disagreeable business for all of them. The animals were examined every day one by one, and such of them as were unable to go to pasture were fed in the sheep-cot, where the forage was carefully spread out for them; because the sick animals had neither strength nor inclination to pull it out of the racks: it was necessary to renew the litter often, and to perfume the sheep-cot several times a-day; a precaution which prevented the smell from becoming insupportable to those who dressed the sores.

This was not all. The lambs made their appearance before we had overcome the disease; several of the poor sheep miscarried, or produced lambs which were so weakly that they could not live; others of the lambs died for want of milk, and those which survived took the disease; all which increased our difficulties.

The disease raged with all its violence for three months,

 and

and during a whole year many of the animals continued lame. If we calculate the loss of the animals which died of the disease, the loss of the lambs, and the great expenses attending so tedious a cure, we may be convinced that the scab itself, terrible as it is, is a less troublesome malady than the foot-rot when it is contagious and general in a flock.

Before pointing out the method of preventing and curing this evil, I shall mention a fact which will show how far it is contagious, and of how much consequence it is to increase our precautions in order to get rid of it. The rams who were upon the mountain at the same time with the diseased flock, took the foot-rot. They were separated from the rest of the diseased animals; and at the end of four months, after having passed through all the usual operations, they appeared to be cured. They still had tender feet, however, and walked with pain; but as the hoof was well recovered, and there was no appearance of ulceration upon it, they were driven to the neighbourhood of a Spanish flock. They were placed under a pent-house, separated from the sheep-cot by a wall. Some of these rams continued to eat out of the rack upon their knees, which we attributed to the sole of the foot not being yet consolidated; but at the end of fifteen days we perceived that an oozing out of purulent matter had again commenced at the juncture of the horn of the hoof. They were then transported into an infirmary, to be submitted once more to the same treatment. The straw upon which they had lain was not taken away; and the Spanish flock having afterwards been sent into this pent-house, the foot-rot began to show itself among them in about fifteen days. The vigorous measures and precautions I followed, and the treatment I am about to recommend, hindered the disease from proceeding any further in this flock than the second degree; otherwise I do not believe that a single beast would have escaped.

Precautions and Treatment.

At all times, upon receiving a strange flock, it is advisable to keep them separated until it is well ascertained that they

they are not infected with the scab or any other contagious disorder.

The precaution is not less proper in the case of the foot-rot; for although there may be no crippled animal in a flock newly come to hand, yet there may be one among them which had been imperfectly cured during the journey, and in which the disease may break out anew. If there are any actually lame at their arrival, they must be carefully examined. Sometimes it happens that they may chance to be crippled from some other cause than that of foot-rot. On a journey in a moist season the clay sometimes gets hard between the hoofs, and thereby lames the animal. A single glance will suffice to see whether this is the cause of the lameness. Sometimes they are crippled in consequence of the gland between the hoofs being swelled: this is cured of itself, or at the worst by cutting off the gland, and it is not contagious. At other times the animal is crippled merely from fatigue; for which a little rest is the obvious cure. But if the district from which the sheep come is suspected, all diseases of the feet must be examined more cautiously than usual. A heat in the foot is a certain sign of an abscess existing in the hoof, to which an outlet should be given. The animal must then be carefully separated from the rest, and the operation performed which I am about to describe.

If the ulceration is visible, the place must be cleaned with a rag, and Goulard-water laid upon the sore by means of a feather, or the powder of blue vitriol. In order to prevent any dirt, &c. from getting into the wound, the diseased foot should be placed in a little boot, the sole of which is of leather or felt, and the upper part of cloth, in order to fasten it round the leg of the sheep. This precaution is not only favourable to the animal; it also prevents contagion, which seems to be communicated by the pus or sanies which flows from the ulcers upon the litter of the sheep-fold. But when the disease is situated between the division of the hoof, the boot must be large enough to allow the foot to be moved in its natural way; for, if the two divisions were locked together, the disease would fester instead of healing.

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When the disease is seated within the horn of the hoof, it is attended with great pain without any visible disease; the animal does not rest upon the diseased leg, yet it has all the appearance of being well. Upon putting the hand upon the hoof it is found to be very hot; which is easily ascertained by comparing it with the sound legs. We must then endeavour to discover on what side the abscess or interior ulcer is: in order to do this, the foot of the animal must be slightly pressed with the thumb all round the junction of the horn with the skin, as well as the sole of the foot. The seat of the abscess may be easily ascertained by the wincing motion of the foot. This is the place which must be cut with a keen-edged knife, so as to occasion the discharge of the matter and lay the flesh bare. When the wound has bled for some time, a feather, wet with the water of Goulard, is laid upon it, and the boot above described is put on.

It sometimes happens, that upon pressing the foot with the finger no place can be fixed upon as being the seat of the disease: this is the case when the abscess is seated below the thickest and hardest place of the hoof. In this case it is necessary to make large incisions, sometimes without any benefit, before finding the disease; and after waiting a day or two the matter of the ulcer begins to appear, and eats through the horn, in descending to the sole, which then becomes painful at the place where it is necessary to make the incision.

In general we need not be afraid of cutting into the quick and bleeding the diseased feet. The horn of the hoof grows again with singular expedition. I have often seen feet which were completely unhoofed; others, of which part only of the horn was taken away, which healed much sooner than such feet as were scarcely ulcerated.

It would seem that in this disease, the juices which administer to the reproduction of the horn or hoof exist in greater abundance in the above places in disease than in health. When the disease is neglected, and where the sole of the foot has been gnawed off and the whole foot ulcerated, I often found that the sides of the horn had sent out cross

slips

slips from one side of the sole to the other, thereby becoming a sort of boot, on which the animal rested without much pain. Sometimes also the horn in growing again assumes uncommon shapes.

The dressing must be carefully repeated every day with the greatest regularity. It consists in removing the boot and cleaning the wound with Goulard-water. The other feet of the animal must be examined as well as the diseased one; for the disease often passes from one foot to another, and it is sometimes visible to the eye before the animal is lame in the foot recently attacked. Some drops of Goulard-water will then prevent the progress of the disease. When the disease is taken in time, five or six days are sufficient for the cure. If a good deal of the horn has been removed it will require a longer time, until the horn has grown again, and assumed sufficient consistence for the animal to walk without being crippled.

As long as the least matter is perceived and the wound is not dry and cicatrized, even although the animal is not lame, it must not be thought cured, for it will carry back the contagion to the flock from which it had been separated. It must not be allowed to pasture with the rest until completely healed, and even then all its four feet ought to be bathed with vinegar for a few days at first.

Unfortunately this malady is subject to frequent returns. I have often seen animals which appeared to be well cured and walked perfectly well for fifteen days, and then were again seized. Those which have already had it, so far from being less subject to it, are more exposed to it. This happens from the nature of the treatment. The remedies I have prescribed can only check the progress of the disease; and until we have discovered a purifying specific, we may often see the disease reappear several times on the same animal.

It is of great consequence to be extremely vigilant in placing the animals in the infirmary, and in taking them away in proper time. In the season when sheep do not leave the fold, the lame ones are not easily discovered, and sometimes not until the disease is of some days standing,

so

so that the disease may have been communicated to many others before the diseased animal is taken away.

If the least degree of infection is supposed to exist, they ought to be walked up and down every day in an inclosure, in order to observe if any of them are lame. It is also necessary to remove them from the infirmary as soon as the ulceration disappears, because they may take the disease again from those around them. Fumigations of nitric acid are salutary for preventing the smell, and may also hasten the cure of the ulcers. The litter should also be frequently changed; and when removed it must not be left in a place where the healthy animals are liable to be exposed to it.

When the precautions are resorted to, and the care taken which I have described, there will be no danger that the disease will assume any serious appearance.

Every thing pertaining to the knowledge of this disease, which is absolutely new in France, and I have reason to believe unknown in Spain, is extremely important to the proprietors of flocks of Merinos or mongrels. I hope that those who are in possession of any new facts on the subject of the foot-rot will publish them. I obtained from a professional man of Piedmont a succinct memoir concerning this disease, and I shall here insert it.

" Sheep, and particularly those with the finest wool, are subject to a contagious whitlow, which hinders them from pasturing, and which, on account of the pain and the suppuration which it occasions, gives them a continual fever, which increases in the evening. They fall off in flesh, and lose their wool; the rams lose the appetite for copulation, the mothers lose their milk, the lambs are weak and die of consumption.

" There are three kinds of whitlow, which succeed each other. The first is seated under the epidermis, between the two divisions of the foot; the animal is seen to halt: if we lay hold of the foot it feels hotter than usual, and it has a bad smell. Upon examining the place, an oozing out of matter is discovered.

" The second species of whitlow is seated under the horn. In

In this case the lameness and the heat of the foot are greater, as also the degree of fever.

"The third species attacks the phalanges or the bones of the foot, and is caused by inattention to the two former stages of the disease. The cure of this last is very troublesome and difficult.

"The disease arises from long journeys, pasturing in marshy places, allowing the sheep to mix with swine, or from lying in damp folds without litter.

"*Preventatives.*—1st, Remove as much as possible the above causes: 2d, separate the diseased from the healthy animals the instant the infection appears.

"*Cure for the first stage of the complaint.*—As soon as the shepherd perceives the disease, he must dry the place affected very carefully with a linen rag, and spread over it vitriol of copper in powder.

"In the second species of the whitlow, it is necessary to cut off that part of the horn which is detached from the phalange. We should begin cutting at the point of the horn and proceeding upwards. This operation must be performed by paring successively thin slips off the horn; when the horn is completely removed and the flesh bare, the receptacle of contagious matter is discovered. Sometimes it has gnawed very deep, and then the ulcer must be cleaned to the very bottom, by continuing to cut by little and little.

"In order to clean the wound thus laid bare, the foot must be plunged into water heated to such a degree that we can scarcely hold the hand in it. The diseased foot must be plunged and replunged in this hot water several times, letting it remain only a few seconds at each time in the water. It is then dried with a cloth, and a feather dipped in muriatic acid is drawn over the place. The animal must be kept in a fold where there is plenty of straw for 24 hours. Next day it may be put out to pasture where there are no stones nor thorns. Every night the feet of the animals must be inspected; and if ulcers are again formed the treatment must be renewed. They must be always dressed in the evenings, because the repose during the night greatly contributes to the good effects of the remedies.

"The

"The whitlow of the third species is very difficult to cure: the horn must be cut, and the flesh taken off also; and the carious bone must be then scraped, and seared with a red-hot iron."

The manner of operating with the knife, in order to discover the seat of the disease, is extremely well described in the above memoir. The analogy between the treatment of the whitlow in human creatures and that in animals, shows how efficacious the immersion in hot water is as recommended by the author; and the careful cleaning of the ulcers, upon which he insists, is extremely important. I entreat that some intelligent agriculturists may communicate to the public their observations from time to time on this disease, and the best methods of cure.

XXXVI. *On two Species of the Sphex or Wasp, found in Virginia and Pennsylvania, and probably existing through all the United States.* *By* H. LATROBE, *Esq.*

THE two species of sphex whose astonishing industry presents such interesting particulars, are known in America by the names of the *blue wasp*, the *mason* and the *dirt-dauber*. These wasps are distinguished among all the remarkable insects which belong to the order of the *hymenopteræ* of Linnæus, by the singular and cruel manner in which they provide for their young.

The two species of sphex now mentioned are distinguished from each other by their manner of building, and by the form of their bodies; but they are quite similar in their manners, in the materials they employ in making their cells, and in the food they prepare for their progeny.

The first is probably the *sphex cærulea alis fuscis* of Linnæus *. It is by far the most common: its feelers are sharp-pointed, and are extended when the insect is at work; on its snout it carries a strong beak, with which it works sideways, by making furrows on the surface of its little

* The blue ichneumon wasp, with gilt wings. (De Geer.)

cells,

cells, which appear as if channelled; its thorax is thick, and the abdomen is attached to it by a kind of slender stalk like the petiole of a flower. To this petiole belongs a scutum from which issues a strong hook, very useful to the animal in securing its prey. The sting is not very painful, and the pain of short duration. The wings, (which Linnæus describes as being brown,) besides being of a fine green, are also blue and brown. The joints of the feet are yellow, and the whole head, body and legs are of a blue colour. The writer of this article has seen some individuals which had yellow spots upon the thorax at the root of the wings.

The other wasp * (*sphex nigra, abdomine petiolato atro, alis subviolaceis*, of Linnæus,) has a large head, a flat and open nose; the thorax longer in proportion, the petiole of the abdomen very long, it has no hook; the abdomen is conical and of an elegant form. Its colour in general is a deep blue approaching to black, but there are plenty of yellow spots upon the thorax; the thighs, legs, and feet are also spotted with yellow. Its feelers are longer than those of the preceding one; it carries them vertically, and crooks them often.

The cells of both species are built of clay, which the insect collects in moist places; but the appearance and construction of these cells are different for each species.

The blue sphex chooses in the open air the south front of a rock, or trunk of a tree, for its residence. It then seeks its building materials on the bank of some rivulet: it collects the clay with its feet; and after having made as large a ball as it can carry, it begins by laying a slender coating upon the wood or stone. It spreads the clay with its head, and a sharp sound is heard while it is at work. It then flies off for another load, and soon forms the upper extremity of its cell. It then goes on to a second range, working alternately on both sides, and often visiting the interior of the tube, which it renders perfectly close and compact. It thus forms a funnel three or four inches long before attempting to lay up any provisions for its young.

* Ichneumon wasp of Pennsylvania. (De Geer.)

In

In the inside of a house the wasp finds no place so convenient to build its nest as the back of a picture, because it prefers establishing itself in places where there is not too much light; and the back of a picture has also the advantage of furnishing two walls to its cell. The hollow mouldings in a pannel retain it strongly, as well as the interior angles of a table. In the wooden houses of Virginia, such places swarm with their nests.

I have seen the empty space between the top of the books and the upper shelf of a bookcase occupied by a whole family of these wasps, which had saved themselves a great deal of trouble in this instance, as they had only to build one division in their nests.

The nests of the Pennsylvanian wasps differ essentially from those of the *sphex cærulea*. In place of long tubes divided into distinct cells, the former construct horizontal chambers contiguous to each other. They are completely polished within, but are more coarsely wrought without.

Both species of these insects, however, prepare the same kind of food for their young; that is to say, spiders of every kind, but especially those which do not secure themselves by very extensive webs. It is a kind of yellow spider which the wasp collects in greatest quantities. The author, however, has seen both species attack very large spiders in the middle of their webs, and surrounded with the carcases of the insects they had devoured; he has even seen one of these wasps dart quickly upon the spider and wound it with its sting. The wasp then retired to clean itself from some fibres of the web; which it did like the common fly, by brushing its wings and head with its legs. After having been attacked several times, the spider tried to effect its escape by dropping quickly down by means of its thread to the floor, when it began to run off; but its antagonist continued to sting it, and even attempted to carry it off: the spider was, however, too large and heavy; and although the wasp tried to lighten the weight by cutting off the spider's legs, it did not succeed in carrying off its booty for a whole hour, during which time the author was watching.

The

The insect does not kill the spiders which it collects in this manner, but leaves enough of life in them to prevent them from putrefaction or from drying up. In all the cells that I opened, I found the spiders in a state of languor, which admitted of their moving their limbs without changing their places. We can conceive nothing more painful than their situation: they are huddled together for the purpose of being devoured piecemeal by the young wasps, for whose food they are destined.

Each of the cellules of the Pennsylvanian wasp, being intended to contain a certain number of spiders, is separately constructed; but the *sphex cœrulea*, which builds a long tube, gathers as many spiders as it thinks necessary; and, after having laid an egg, encloses it along with the spiders by means of a transverse division of clay. It lays another egg in the following cellule, which it fills and shuts up in the same manner, and so on with four or five cellules in the same tube.

The egg is not long of hatching after being closed up; but the author was not able to ascertain the time required for the formation of the young wasp. There are drawings coloured after nature published with the memoir, giving sections of the cells of these wasps, and showing the different periods of the transformation of the insects.

As I always found an unequal number of spiders in various cells, but apparently proportioned to their capacity, I opened a range of the cells of the Pennsylvanian wasp; and having weighed separately the contents of each, I obtained the following results:

	Grains.
In the first cellule the spiders weighed -	$7\frac{1}{2}$
In the second, there were 17 spiders and an empty skin; the worm weighed $\frac{1}{4}$ grain, and the spiders	$6\frac{1}{2}$
The third contained 19 very small spiders and some empty skins; the whole weighed - -	$5\frac{3}{4}$
The worm weighed - - - -	$1\frac{1}{2}$
The fourth contained only carcasses of spiders, the worm was weak and feeble. I presume that it had too little nourishment, or that it was sick; it weighed - - - - - -	$3\frac{1}{4}$

The

The fifth contained an envelop in which was a large worm not yet in the state of a chrysalis; the whole weighed - - - - - Grains. $3\frac{1}{2}$

The sixth and seventh cellules were empty; the young wasp had abandoned them.

This examination proves that the wasp distributes with much judgment the quantity of food necessary for its progeny; in most of the cellules, for instance, I ought to have found twenty-two or twenty-three spiders, and yet sometimes there are only five or six, but in this case they are very large ones. It appears also, that when the worm has attained its greatest size, its weight is only one half of that of the food it has consumed.

If it should become necessary to break through the barrier antiently traced between reason and instinct, the œconomy of the whole class of *hymenopteræ*, and particularly of the wasps, may contribute to it. I shall relate a singular example which appears to be above mere instinct.

For the purpose of inspecting one of these insects (the Pennsylvanian wasp) while at work, I was obliged to remove a small distance from the wall a picture behind which the nest was placed. In doing so I deranged several cellules, because the earthy mastic which joined them to the wall was broken in several places and exposed the spiders and the young worms to view.

I held the frame about an inch from the wall in order to see what passed behind. In a short time the wasp arrived, loaded with a round lump of clay. It came merely for the purpose of making a new cellule; but seeing that its former works were deranged, it began to run rapidly over the cellules, apparently hesitating what to do. At last it deposited the clay upon the edge of one of the apertures, and began to spread it with its snout, pushing it before it, in the attitude of a sow digging in the ground. It emitted a shrill buzzing when at work. After having very properly replastered the work, it flew away. In four minutes it returned with a new load of clay, which it deposited in the next aperture. It repeated its visits four times; and after having finished the repairs and being convinced of the

 goodness

goodness of the workmanship by running over it several times, it flew off again and returned with a new load, with which it began to form a new cell.

If the faculty of modifying the conduct of an individual according to circumstances is one of the characteristics of reason, the fact I have now mentioned is surely a proof of reasoning in an insect. The wasp had remarked the unexpected derangement which had been made during its absence; the clay which it brought was intended for a new cellule; but observing the mischief done to the old ones, it repaired them before building any more.

XXXVII. *Description and Manner of using* M. MONTGOLFIER'S *Calorimeter, an Apparatus for determining the comparative Quantities of Heat furnished by various Descriptions of Combustibles* *.

THE proper application of combustibles is one of the most important objects in all the processes of the arts, more particularly in chemical operations; it is equally useful to ascertain what advantage in point of œconomy results from the employment of this or that kind of fuel, and to determine the force of the caloric disengaged from the substances which we burn.

Equal quantities of different kinds of combustibles do not always yield the same degree of heat; and it requires a longer space of time to disengage a given quantity of caloric from some combustibles than from others. The success of an operation very often depends upon the promptitude with which it is executed. Manufacturers, distillers, and agriculturists ought consequently to attach a great deal of importance to the knowledge of the most œconomical fuel, or what are the effects which result from a given quantity of any particular kind; in short, it ought to be distinctly ascertained what is the most certain and the easiest method of determining the difference of the action of caloric.

* From *Journal des Mines*, vol. xviii.

M. Montgolfier, to whom we are already indebted for many useful discoveries, has put an end to all difficulties upon the above subject, by the invention of an apparatus to which he has given the name of a calorimeter *.

Description of the Calorimeter.

Plate V. represents a section of the above apparatus.

A, B, C, D, is a case of tinned iron (if made of wood it would be more œconomical and advantageous) which must be so well joined that no water may be permitted to escape; it has a cover A, B, through which is made an aperture *a, b*; in the bottom also is an aperture *e, f.*

The small stove *a, b, c, d, e, f,* is made of plate iron or copper, having its joinings carefully closed that it may not admit of any of the water getting in. Its lower aperture corresponds with that of the case *e, f*; another aperture in the upper part is closed near *a, b,* by a plug, which may be taken out at pleasure.

c, d, is a grate composed of iron wire upon which the combustible is placed; the ashes fall down from the grate by the aperture *g*.

Near *h, i,* a pipe *k, k,* is adapted for giving a passage to the smoke which escapes by the aperture *l*. This pipe ought to be made of plate-iron or copper, and so close as not to permit the water which surrounds it to penetrate it.

m, m, is a pipe of plate-iron larger than the former and which surrounds it, so that the water is kept between both.

E is the reservoir, of which the covering *r, s,* may be taken off to fill the apparatus with water.

o, o, is a pipe which comes from the same reservoir, and which communicates with the pipe *m, m.*

p, is a cock to let out the boiling water; and *q,* is another cock, by means of which we may empty the apparatus if necessary.

F. G. are the feet upon which the apparatus rests.

* This apparatus differs essentially from the calorimeter proper for chemical experiments invented by Messrs. Lavoisier and La Place.

Method

Method of using the Calorimeter.

When it is necessary to determine the length of time which various combustibles require to extricate a given quantity of heat, the reservoir E is filled with water, which passes through the pipe *o, o,* ascends into the pipe *m, m,* and penetrates by the pipe *n, n,* into the case A.B. C. D. As much water is poured in as will fill the whole of the case, and when full it maybe easily seen by the water not descending below the line *t, u,* which is the highest point of the water in the apparatus, and its temperature is observed by means of a thermometer. A sufficient quantity of the combustible to be used in the experiment, for instance, wood, cut into small pieces, is then placed upon the grate *c, d:* after having lighted it up, the upper opening of the stove *a, b,* is closed with the stopper, and it must then be noticed how long time it requires before the water arrives at a certain degree of heat (the boiling point for instance), and which may be ascertained with the thermometer. The fire is then withdrawn, and the water as well as the stove are allowed to cool until they return to the temperature at which the operation was commenced. Another kind of combustible, suppose coal or turf, may be then placed on the grate, and the apparatus managed as before, after the fire being lighted up.

We can ascertain the greater or less promptitude with which heat is extricated from the combustible, by comparing the length of time which the two experiments required.

To find the difference of the quantity or weight of combustibles of different kinds proper for producing this temperature, it is necessary to take a sufficient quantity of one of the combustibles, suppose a cubic foot of wood for example;—it is lighted up in the stove after the apparatus is filled with water, and its temperature marked. The thermometer determines the moment when the water is in ebullition. The fire is then extinguished, and all the combustible remaining in the grate is taken away; and when the apparatus has returned to the first temperature, any other combustible may be treated in the same manner.

If after the operation the quantities of combustibles em-

ployed are compared with each other and valued at a mean price, it will be easily seen what effect the one has in comparison with the other, and consequently what species of fuel is the most œconomical and cheapest.

We may observe further, that the pipe *m* may be made of wood; but if it is of plate-iron or copper, it must be surrounded with several coatings of paper, by which means less heat will be lost.

The pipes *k*, *k*, and *m*, *m*, may be lengthened at pleasure, because still a good deal of caloric escapes by the aperture *l*.

This apparatus may be applied to various purposes; such as boiling water at a small expense. It is of great utility in domestic œconomy. To render its effect as complete as possible, the smoke, or rather the burnt air, should be deprived as much as possible of its caloric, which ought to be employed entirely in gradually increasing the temperature of the water surrounding the chimney. This air, thus cooled, being heavier than that of the atmosphere, determines in the furnace the current of air; an effect which is not obtained in ascending chimneys but by sacrificing a very considerable quantity of caloric. It is consequently advisable to lengthen the chimney as much as the height of the apartment permits *.

* We very much suspect that this apparatus has never been tried; for, in the way prescribed for conducting the experiments, it would be difficult to obtain the same results twice from the same kind of fuel. Even in an open grate it would be no easy matter to find out means by which to make a fire of wood, of coal, or of turf, always burn up *in the same time* and *with the same degree of force in the same time*. The difficulty, nay, the impossibility, of always laying the fuel and igniting it in such a manner that all the circumstances shall be precisely the same, renders it very improbable that results should ever be obtained that could be at all depended on, by following the means pointed out. Still, however, we think the instrument, or something similar, might be so employed as to give useful results—not by such short experiments as are proposed in the above paper, but by carrying on the trials with each of the different kinds of fuel for a day or two, and ascertaining how much water a given weight of each of them could evaporate, and noting the times.—Edit.

XXXVIII. *Che-*

XXXVIII. *Chemical Observations upon Spathic Iron. By* M. Collet Descostils, *Engineer of Mines. Read at the Class of the Sciences in the French National Institute, January* 6, 1806*.

I think there is no mineral substance the various analyses of which present so many important differences as those of spathic iron, and the treatment of which, in some forges, agrees also so ill with the opinion given of its composition in the most of these analyses. After having examined the processes employed by the chemists who have published them, and having made some experiments upon the same subject, I think I have discovered the cause of the different results which they announced, or the errors which they committed. I shall explain in this memoir the results of my labours, and I shall conclude it by some conjectures upon the causes of several metallurgical practices made use of in the treatment of spathic iron, and which I think have not been hitherto satisfactorily explained.

Bayen, I presume, was the first chemist who analysed spathic iron. His work, which appeared to me to contain some errors, presents, however, some unquestionable results which seem to have been forgotten, and which I think I ought to bring to recollection.

Distillation and the action of the acids convinced M. Bayen, 1st, That this ore contains a gas of the nature of fixed air, to which he attributes the property of crystallizing the iron. 2d, That it contains fragments of quartz, which remain in the liquor after its complete solution, if it has been exposed in small portions to the action of the acids.

The crystallization of some sulphuric solutions, without excess of acid, sometimes presented small quantities of sulphate of lime, at other times he did not perceive an atom of it; and he concluded from this, that the above ore, considered in the mass, is found in some places mixed with calcareous spar.

The other experiments he relates, and the consequences he

* From *Journal des Mines*, vol. xviii.

deduces from them, do not appear to me to be so exact; in as far as he asserts that the calcined ore was attracted by the magnet, that it was dissolved in nitric acid with facility, and, according to his account, with a very lively effervescence; and that it reduced minium, and, lastly, served to decompose cinnabar: he concluded from all these circumstances, that the iron in this ore was in the true *metallic state*. This inference is obviously so inadmissible that we need not dwell upon it.

The following experiment deserves to be described. After having treated the ore with nitric acid and evaporated it to dryness, he washed the residue, and obtained from the liquor by means of fixed alkali a white earth, which he says he found after repeated experiments to be calcareous earth. We shall soon see the reasons which I have for doubting the accuracy of these experiments, as well as his discovery of zinc in a remarkable quantity in this ore. The small black flakes which he perceived in the muriatic acid which had been poured upon this ore, made him think that this metal existed there. To convince himself of it, he digested in the cold for ten or twelve days some sulphate of iron dissolved in a small quantity of water, upon spathic iron calcined and well pulverized, taking care to agitate the mixture from time to time; he afterwards filtered it, and obtained by evaporation, and allowing it to subside, crystals, which appeared to him to be sulphate of zinc.

This result is without doubt very remarkable. M. Dizé also judged it necessary to verify this fact upon the same specimen which Bayen made use of in his experiments. M. Dizé, in order to obtain oxide of zinc, employed the action of nitric acid and precisely the same kind of evaporation which Bayen made use of to extract this calcareous earth, which he had ascertained, as he says, by decisive experiments. The experiment of M. Dizé was made upon fifty grammes: he obtained by means of a fixed alkali five grammes of a white substance of an earthy appearance, which he mixed with double its weight of charcoal, and distilled in an earthen retort. Upon the apparatus cooling, particles of zinc were found sublimed at the top of the retort.

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This experiment is without doubt decisive; but does not the small quantity of zinc obtained in the distillation prove that the zinc was in very small proportion in the earthy substance? This supposition appears to me to be confirmed by what M. Dizé himself says of the action of prussiate of lime upon the nitric solution, and without excess of acid, before it had been precipitated by the alkali:—*it had a saline taste; the prussiate of lime did not injure its transparence;* and, he adds, *the oxalic acid and sulphuric acid do not demonstrate the presence of lime.* Besides all this, M. Dizé had nothing in view but to prove that the specimen of Bayen contained zinc. I repeated the same experiment upon a piece of spathic iron from Vaunaveys, and I obtained the results of M. Dizé, with the exception of the zinc, of which I did not perceive the least traces; which proves, that if some of these ores contain a little, at least they do not all contain it.

According to what I have said, we may consider the following as the certain results of Bayen's experiments.

1st, That the iron is combined in the spathic iron with carbonic acid.

2d, That the quartz and the calcareous carbonate sometimes obtained in the analysis of this ore do not enter into its composition.

Almost at the same time with Bayen (in 1774) Bergman published upon the white ores of iron; he proves in this work, that the iron in these ores is in the same degree of oxygenation as in green vitriol: as for lime, although he obtained some very great differences in the quantities which he procured from different specimens, he determined to regard it as a constituent part of spathic iron. The method which he employed in order to extract it, consisted in calcining the ore, reducing it to fine powder, and agitating it afterwards a long time with diluted nitric acid. The fixed alkali was then employed in order to precipitate from this acid a white earth, which he ascertained to be lime. I do not think, however, that this result can invalidate the conclusion which Bayen deduced from his experiments. In fact, it may be rightly judged, that in a long course of labours of

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the same description, all the products have not been examined with the same care; and it may be possible that some of the varieties may have contained other earthy substances which may have been considered as lime; a circumstance which might have induced him to say, that he never found spathic iron entirely free of it.

Bergman, in making known the existence of manganese in the same mineral, and that it was to its strong proportion in spathic iron that the property was owing which this ore has of yielding steel, has announced a fact of great importance, and his opinion has been regarded as almost proved.

If some doubts could have been raised upon the exactness of this result, they could only have been relative to the quantity of manganese; but we know that nitric acid and sugar, employed by Bergman to separate the manganese of iron, are very inexact methods.

M. Sage, in his *Analysis and Concordance of the three Kingdoms*, admits no lime into spathic iron; but he acknowledges the presence of manganese in the same proportion as the Swedish chemist, and he says he obtained the sulphate of this last metal in white prismatic tetrahedral crystals, which crystallized *before* martial vitriol. This form is that of the crystals of sulphate of zinc, and made him believe at first that this last metal existed in spathic iron; it was, without doubt, the same form which Bayen had observed, and which made him draw the same consequence. The last opinion of M. Sage appears to me to be susceptible of some objections. In fact, if these crystals are owing to manganese, they would have another form; they would not be white, but slightly red: in short, they would not crystallize except *after* sulphate of iron, that of manganese being much more soluble.

M. Bucholz published, about a year ago, in the *Journal Allemand*, edited by Messrs. Klaproth, Hermstadt, &c., the analysis of a mineral, which, from the description he gives, is easily recognized as spathic iron. He there announces 59½ in 100 of iron in the state of black oxide, and 2,5 of lime, which he seemed to regard as foreign to its composition.

composition. The remainder is water and carbonic acid. It is worthy of remark, that the proportion of iron is determined according to those of the other principles, all of which were examined with care, by deducting their amount from the quantity of ore submitted to analysis. Consequently the losses made upon these principles would correspond to the augmentation of the proportion of the iron; we must therefore suppose, that this proportion is a little too high in the result of this analysis.

M. Bucholz searched for the manganese particularly, but he did not discover an appreciable quantity of it: besides, he employed so many different methods, in order to ascertain that this ore contained no other substance, either earthy or metallic, that we cannot accuse his results of error.

M. Bucholz also observed, that in the calcination of this ore, the carbonic acid is in part decomposed; and that a gas is evolved which burns blue, and which he designates by the name of gaseous oxide of carbon.

Lastly, M. Drappier, in a memoir lately published*, has announced, that the specimens of spathic iron which he examined did not contain any lime; that they contained only a very small quantity of manganese; but that he found magnesia in a very great proportion. Before proceeding any further, I shall mention two analyses of M. Bergman, related by M. Hassenfratz in a note printed in the same paper. As the latter had it principally in view to make known the quantity of lime contained in spathic iron, and as the proportions obtained from the two specimens have been very different, we may again conclude that this substance is foreign to spathic iron. We may draw the same consequence from a third analysis reported in the same note, and which appeared to have been made upon a specimen of roasted ore, although water is mentioned in it. This is certainly occasioned by some mistake of the press or of the editor.

The little conformity between the analyses of Bergman and those of Bucholz and Drappier; the existence of the oxide of manganese in great proportion, announced in such

* For a translation of M. Drappier's paper, see p. 31 of the present volume.

a positive

a positive manner by the former, and not recognised by the latter; and, above all, the existence of magnesia, in variable proportions, announced by M. Drappier, while no other chemist had spoken of it, must leave our opinion uncertain upon the exactitude of these analyses. A desire to explain the doubts which might be raised upon these very important facts relative to metallurgy, determined me to examine this species of ore again, by taking specimens which presented differences among them.

I consequently chose, out of the fine collection of the Council of Mines, two pieces of spathic iron; one of them was from Vaunaveys, the same from which M. Drappier had detached some fragments; the other was from Allevard, but very different in its characters from that which M. Drappier had received from M. Hassenfratz. I shall subjoin the characters of both.

Specimen from Vaunaveys.

Specific gravity 3·6.

Colour brownish yellow.

Semitransparent.

The fracture laminated and glittering.

The laminæ perfectly smooth, like those of calcareous spar.

Specimen from Allevard.

Specific gravity 3·84.

Colour grayish.

It is opake.

The fracture is sufficiently glittering.

Its crystallization is very confused, and its lamina often rounded: the divisions of the fissures which traverse this specimen have often a very black superficial colour, and which has scarcely a metallic lustre. I chose for the analysis the gray portions pretty well freed from the brown *.

The process which I followed consisted in attacking, by the nitric acid, the ore reduced into fragments, in after-

* By the dry method, with an equal weight of vitrified borax and a little oil, in a crucible lined with charcoal, to 100 parts of crude ore, 34 was yielded from the ore of Vaunaveys, and 37·6 from that of Allevard, on the first essay, and 38·2 in the second.

wards

wards evaporating to dryness, and in redissolving with water the salts which were not decomposed. The oxide of iron was collected upon the filter. I afterwards precipitated the manganese with the prussiate of potash; and I obtained, with the assistance of a fixed alkali, the earthy substances contained in the clear liquor. Last of all, I separated the lime from the magnesia by means of sulphuric acid and evaporation. It is to be observed that the nitric solution must not be evaporated too strongly: without this precaution we run the risk of decomposing a great portion of the nitrate of manganese. I examined, by several methods, the products of each experiment, in order to avoid errors as much as possible.

I subjoin an account of the products obtained from each analysis.

	Vaunaveys.		Allevard.
Fragments of quartz	0		2
Red oxide of iron -	49		50·5
Red oxide of manganese	1·5	from 9 to	10
Magnesia - -	12·5	at most	2
Lime - - -	0·3		0·5
	63·3		64·
Loss by calcination	37·5		34·5
	100·8		98·5
Excess	8	Loss	1·5

These results do not indicate the exact proportions of carbonic acid which exist in these spathic irons, because one part of this acid is decomposed during calcination, and there results from it a much stronger oxidation of the metal, in such a manner that the loss which the ore suffers, and which might be ascribed to water and carbonic acid, is a little below the real quantity in relation to this last principle; but as, on the other hand, the iron in the ore is in the state of a green oxide, and as I have obtained it in the state of red oxide, the augmentation which results from it compensates for the low estimate of the carbonic acid; and in the last result the real loss is a mere trifle.

[To be continued.]

XXXIX. *De-*

XXXIX. *Description of an Improved Crane and Flexible Chains.* By Mr. GILBERT GILPIN, *of Old Park Iron-Works, near Shifnal* *.

HAVING discovered a method of working chains of the common construction, over pulleys, in all directions, more safe and flexible than the best hempen ropes, and at the same time equally uniform, I have sent, for the inspection of the Society for the encouragement of arts, manufactures, and commerce, a full-sized pulley in wood, and a piece of a chain, together with a model of a crane, exhibiting its manner of application.

From its simplicity of form, and facility of manufacture, the common chain, formed of oval links, has been in use from the earliest ages; and that it did not answer every purpose of a hempen rope, in working over pulleys, was not owing to its peculiar form; but from an error in the application.

Every chain of this nature has a twist in itself, arising from a depression given by the hammer to each link in the welding †; and this circumstance, so trifling in appearance, is not so in its effects, and it has in consequence a perpetual tendency (even when reefed perfectly straight in pulleys, and on the barrels of cranes) to assume a spiral form, which a plain cylindrical barrel, and the common pulleys with semicircular grooves, are not in the least calculated to prevent. Hence the alternate links of the chain, in coiling round a barrel, or working over pulleys, form obtuse angles in assuming the spiral form, bearing upon the lower parts of their circumferences, and forming as it were two levers, which wrench open and crush each other in proportion to the weight suspended, as well as prevent the freedom of motion in the links themselves, and thereby load the chain with additional friction.

* From *Transactions of Society of Arts*, &c. 1806. The silver medal of the society and 30 guineas were voted to Mr. Gilpin for this invention; and models of the crane and chain are reserved in the Society's Repository, for the inspection of the public.

† The twist may be seen by holding the piece of the chain by one end, and viewing the links edgeways as it hangs down.

A still

A still greater obstruction to the uniformity of its motion, is the tendency which the chain has to make a double coil as it approaches the middle of the barrel and crosses its centre, and that of the pulleys at right angles, by means of which the chain is frequently broken by the sudden jerk caused by the upper coil slipping off the undermost.

It is to these causes that all the accidents that occur to workmen and machinery from the failure of chains may be attributed (bad iron excepted), and which form the sole objection to their becoming a general substitute for ropes.

As a preventive to these evils, I have grooves cast in iron pulleys, of sufficient dimensions to receive the lower circumferences of the links of the chain, which work vertically; those which work horizontally and form the gudgeon part of the chain (if we may be allowed the expression), bearing upon each side of the grooves.

The barrels are also of cast iron, with spiral grooves of the same dimensions, at such distance from each other as to admit the chain to bed without the danger of a double coil; by these means the links are retained at right angles with each other, the only position for free and uniform motion.

The links of the chains are made as short as possible, for the purpose of increasing their flexibility, and they are reefed perfectly free from twist, in the pulleys, and on the barrels, for the same reason.

When applied in blocks, the grooves in the pulleys prevent the different falls of the chain from coming in contact, and render plates between them (as in the common way) totally unnecessary; the pulleys are in consequence brought closer together, the angle of the fall from block to block considerably diminished, and the friction against the plates entirely avoided. Brass guards, with grooves opposite to those in the pulleys, are riveted to the blocks, to prevent the chain getting out of its birth from any accidental circumstance. This method of working chains I first put in practice for Messrs. T. W. and B. Botfield, at these works, in July last; and it is applied in the working of cranes capable of purchasing from ten to fifteen tons; in the working of the governor balls of steam-engines constructed by Messrs. Boulton and Watt,

Watt, and in the raising of coal and ore from the mines, for which purposes ropes had before been solely used at this manufactory. In all cases it has performed with the utmost safety, uniformity, and flexibility; so much so that the prejudices of our workmen against chains are entirely done away, and they hoist the heaviest articles with more ease, and as great confidence of safety as they would with the best ropes.

The same method is applicable, at a trifling expense, to all machines at present worked by ropes, or by chains, in the usual way: and all the common chains now in use may be applied to it with equal facility.

With a view of ascertaining the relative flexibility of ropes and chains, I wedged an iron pulley, thirty-one and a half inches in diameter, on the spindle of the pinion of a crane of the following description, viz.

Barrel, 30 inches diameter;
Wheel, 64 teeth;
Pinion, 8 ditto;
Top block, with three pulleys of 12 inches diameter;
Bottom block, with 2 ditto, ditto.

To the large pulley I attached a small rope, for the purpose of suspending the weights in the hoisting of the different loads, and the results were as follow:

The crane was loaded with	Took to hoist the loads when reefed with the chain in grooved pulleys*	Ditto, when reefed with a half-worn tarred strand-laid rope, 3½ inches in circumference.	Ditto, when reefed with the chain promiscuously as in the common way
lbs.	*lbs.*	*lbs.*	*lbs.*
First . . 2000	63	74	80
Second . 1000	32	39	41
Third . . . 500	17	21	22
Total . . 3500	112	134	143

* All the experiments were tried with the same grooved pulleys.

The

The flexibility is inversely as these momenta, and proves the superiority of chains; for (on the average of the trials) with the chain in the grooves,

One pound raised - - - - -	31·25 lbs.
With a half-worn strand-laid tarred rope, three inches and a half in circumference -	26·11 do.
And with the chain in the usual way, only -	24·47 do.

It also appears (contrary to the general opinion), that chains are safer than ropes; for it is an established axiom, that those bodies whose fibres are most in the direction of the strain, are the least liable to be pulled asunder; and in our examination of the properties of a rope, we find that the strands cross the direction of the strain in undulated lines, and consequently prevent its uniform action thereon. A rope is subject to this inconvenience even when stretched in a direct line, but more particularly so when bent over a pulley, as in that position the upper section, moving through a greater space than the under one, is acted upon by the whole strain; and hence the frequent breaking of ropes in bending over pulleys, from the double strain overloading the strands of which the upper section is formed.

The links of a chain are subject to the transverse strain, where they move in contact; but as such strain is in proportion to the length of the bearing, it must be very trifling. All the links having axles of their own, the chain moves simultaneously with the strain, and both are in consequence retained in continual equilibrio. A chain in grooves will therefore sustain as great a weight when bent over a pulley, as it will in a direct line, and consequently is safer than a rope.

A safe, uniform, and flexible method of applying chains in the working of machinery has long been a desideratum in the arts; for they are but little affected by exposure to the weather, or the heat of manufactories, whilst either produces the speedy destruction of ropes.

The discovery is of additional importance, as it substitutes a durable article for a very perishable one, and gives employment to our own manufactories at the expense of foreign importations.

portations.—The durability is at least six to one in favour of chains.

Though the model of the crane is chiefly intended to convey a proper idea of the new method of working chains, yet I trust it will be found to possess several other advantages in point of construction, which are entirely new, and calculated to increase the safety and durability, as well as to lessen the expense of that useful machine.

On reviewing the principles of a crane, we find that the gudgeons are the points of resistance to the machine and its load, and consequently the effect of the transverse strain upon the perpendicular will be in proportion to the distance of the mortise for the gib from the upper one; and that of the oblique strain, in proportion to the distance of the mortise for the diagonal stay, from the lower one.

Notwithstanding these circumstances are so evident, they are seldom attended to; for in general a large and expensive piece of oak, sufficient of itself to make a crane of double the purchase, forms the perpendicular; the gib is mortised into it, at eighteen or twenty inches from the top, to make room for the gudgeon, as is the diagonal stay, at five or six feet from the bottom, to allow a birth below for the barrel. Thus the effect of the transverse and oblique strains of the gib and diagonal stay upon the perpendicular, is increased by their distances from the gudgeons, or points of resistance, and the perpendicular itself considerably weakened by mortises made where the greatest strength is required. Hence the frequent failure of cranes of the common construction, by the breaking of the perpendiculars in the mortises.

It appears, however, that the various parts of a crane formed of wood, cannot be connected together in any other way than by mortising; and as this method considerably diminishes the strength of the timber, I make use of cast-iron mortise pieces.

The perpendicular is formed of two oak planks, each eighteen inches wide, four thick, and sixteen feet long; these, at the top and bottom, are let into cast-iron mortise pieces, which retain the planks ten inches asunder. The

barrel

barrel for the chain, works between them. The piece at the top contains in the middle a dove-tailed mortise, into which a stock for the gib is fixed; for greater security, an iron bolt goes through the whole; the stock projects two feet from the mortise, and a plank eighteen inches deep, and four thick, is bolted to each side of it to form the gib, the interstice between the planks forming a birth for the top block to slide in. The diagonal stay is of the same dimensions, formed in a similar manner, and connected to the perpendicular, by being let into the lower mortise piece.

In this mode of construction scarcely any part of the timber is cut away; and the strength of the materials, so far from being diminished, is augmented by the cast-iron mortise pieces, the gib is brought much closer to the upper gudgeon, and the centre lines of the perpendicular and the diagonal stay crossing each other at the top of the lower one, places the whole strain as near as possible in a line with the gudgeons. The business of the perpendicular becomes in consequence little more than that of a mere prop, and consequently requires no greater strength of materials than the diagonal stay.

The top block is made of cast iron, and has a groove three inches deep on each side, for the purpose of embracing the planks which form the gib.

To prevent the inconvenience of the dirt of the floor getting into the brass of the lower gudgeon, and thereby obstructing the revolution of the crane, those parts are reverse to the common way, the gudgeon being fixed in the floor, and the socket part which embraces it is cast in the bottom of the mortise-piece, as is also a channel to convey oil to the gudgeon. I am, Sir,

Your most humble servant,

GILBERT GILPIN.

Old Park Iron-works, near Shifnal,
April 16th, 1804.

Reference to Mr. GILBERT GILPIN'S *Crane,* Pl. VI.
Fig. 1, 2, 3, 4.

Fig. 1. represents the crane with all its parts complete, ready for work.

 AB,

A B, the perpendicular, formed of two oaken planks, each eighteen inches wide, four thick, and sixteen feet long, let into cast-iron mortise-pieces C D.

E E, the barrel for the chain which works between the two planks of the perpendicular.

F, the top piece, containing in the middle a dove-tailed mortise, into which H, a stock for the gib, is fixed; an iron bolt goes through the whole, for greater security. The stock projects two feet from the mortise, and two planks I, K, eighteen inches deep, and four thick, are bolted one on each side of it, to form the gib, the interstices between these planks forming a birth or space for the top block L to slide in. This block is made of cast-iron, and has a groove three inches deep on each side.

M, the diagonal stay, is of the same dimensions as the gib, formed in a similar manner, and connected to the perpendicular by being let into the lower mortise-piece D.

N, the handle or winch which turns a small pinion O, fixed on the same axis; this pinion works in the teeth of the wheel P, moving on the same axle as the barrel E, on which the chain R lies in spiral grooves.

S, the block and hook by which the goods are raised.

Fig. 2. is a side view of the handle N, the pinion O the toothed wheel, and the barrel E placed betwixt the two uprights A B.

Fig. 3. shows upon an enlarged scale part of the barrel E, and some of the chain lying in its proper position in one of the spiral grooves, or channels: it is to be noted that the lower edge of one link lies in the groove, and the next link upon the surface of the barrel, and that by this means the chain is prevented from twisting in winding upon the barrel.

Fig. 4. shows a section of part of the barrel E, in order to point out clearly the manner in which one link lies within it, the other link on its outside; it is contrasted by Fig. 7. the old method of working chains.

Certificates, dated November 22d, 1804, from Thomas Blackmore, John Swift, John Ball, Joseph Felton, Benjamin Heylehurst, Benjamin Hunt, and Thomas Hatchless; who declare that they were present at the trial of the experiments above

above mentioned, that they had also seen the new method of working chains in daily use for upwards of sixteen months, and are certain, that in that way chains work much more flexible than hempen ropes, and equally as safe and uniform.

Further certificates from Messrs. I. W. and B. Botfield, lessees of Old Park iron-works, and Isaac Hawkins Browne, esq. landlord of the said works, confirm Mr. Gilpin's statement; and further add, that the method is calculated for chains of all sizes, and for machinery of every description; that it is employed at their works with great success, in the working of cranes and mill machinery, and in the raising of coal and ore from the mines. That his chains applied in this manner are a complete substitute for ropes, and will prevent those fatal accidents which too frequently occur to the workmen and machinery, in the working of chains in the usual way. That Mr. Gilpin's crane is also constructed upon stronger and more durable principles than those in general use, and completely answers its purpose.

XL. *Description of Mr.* PETER HERBERT'S *Improved Book-case Bolt* *.

I HAVE taken the liberty of laying before the society a model of my invention, which I hope is sufficient to explain my intention. I intended it for a library book-case bolt, to facilitate the opening of both doors at once, and to secure the same, without the trouble of bolting two bolts in the common way. It will do for wardrobes, French casements, or folding sash doors. It will also make a good sash fastening, if let into the bottom sash, with a small brass knob to slide as common; it would bolt in the frame by the side of the sash cord, both sides at once. I can also make it to answer sundry other useful purposes if required.

I remain, Sir,

Your obedient humble servant,

PETER HERBERT.

No. 33, Bow-street, Covent-garden.

* From *Transactions of the Society of Arts, &c.* 1806.—Ten guineas were voted to Mr. Herbert for this invention, a model of which is preserved in the Society's Repository.

Reference to the Engraving of Mr. PETER HERBERT'S *Book-case Bolt*, Plate VII. Fig. 3, 4.

KL, Fig. 3, represents the two stiles of the doors of a folding book-case.

M, the key-hole of a lock with two bolts, which are more clearly shown at Fig. 4, where the back of the lock N shows the two bolts of the lock pressing back a sliding-piece O; on the front part of this sliding-piece in Fig. 3, two small friction rollers are placed at P, in the act of pressing against two levers, crossing on one common fulcrum R, to each end of which, shorter levers SS above and below are connected by joints. These short levers act upon two long bolts, whose extremities are shown at TT, having each a helical spring at VV. In the state as engraved, the doors are locked and bolted.

On drawing back the bolts of the lock by means of the key, the helical springs, VV, press against the plates UU, through which the long bolts pass; they force back the long bolts and sliding-piece O, and allow both the doors to open.

XLI. *Notices of Experiments made by the Galvanic Society of Paris. By* M. RIFFAULT, *one of the Members* *.

I.

M. MARECHAUX of Wesel, correspondent of the Galvanic society, announces that he has ascertained that water, whether pure or mixed with an acid, or saturated with any salt, is not a necessary ingredient in the production of the effects of the Galvanic fluid. He adds that for some time past he has constructed columns of zinc and brass, with the interposition of disks of pasteboard not moistened, which were of great service to him. The Galvanic society being interested in a fact of this nature, they determined to repeat the experiments of M. Marechaux, as described in his letter: disks of zinc which had been employed before were

* From the *Annales de Chimie*, tom. lvi. page 61.

completely

completely scraped and restored to their former polish. Similar ones of new brass were made. With the interposition of rounds of pasteboard not moistened, a vertical column was formed of 49 pairs of disks, resting upon a larger plate of brass, pierced with three holes in its edges, through which were made to pass as many silk cords, intended to keep the disks in their position. These cords were tied all together at the top, and the whole column was then suspended from a hook. This pile, which M. Marechaux denominates the pendulous column, having been placed in communication with the electro-micrometer, simplified upon that of M. Marechaux, by M. Veau-Delaunay, it manifested a *tension* * of 360 degrees, which we may be certain was not the effect of atmospherical electricity, but truly that of the Galvanic election.

This first experiment was repeated and varied in different manners. Rounds of blotting paper were substituted in the room of those of pasteboard, to the number of four for each, and no effect whatever was produced. Rounds of pasteboard dried in a stove were also made use of: the mean term of attraction in several trials was 372°. With these same rounds and only 25 pairs of disks, the attraction was 160°. The column was afterwards tried with the same number of pairs of metallic disks, but without the interposition of rounds of pasteboard, and nothing was obtained.

These first results satisfied the society that the fact announced by M. Marechaux was correct; but this Galvanic action, by the pendulous column, could only be ascertained by the assistance of an instrument of great sensibility, and in quantities scarcely appreciable. It remains for the society to ascertain the advantage which may be derived to the progress of Galvanism, by the employment of more powerful means, and by the comparison of the effects produced with piles kept in a state of humidity, by means of saline solutions.

* By the word tension is meant the measure of the distance to which a leaf of gold suspended to a vertical column of copper is attracted towards another horizontal column of the same metal, terminated by a ball, when these two columns are in communication with the two poles of the pile. Each degree of this measure of attraction represents an 18,000dth part of an inch.

tions. The physical class of the society is charged to direct its labours and experiments to this object.

II.

In consequence of Dr. Baronio of Milan having published a description of a Galvanic pile formed of vegetable matters, the society occupied themselves with ascertaining the truth of Baronio's experiments. They procured sixty even disks of walnut-tree wood, two inches in diameter, furnished with a raised edge of about a line and a half, and they boiled them in vinegar for some time. With these disks and some rounds of raw beet-root, and radish, (*Raphanus sativus* of Linnæus) a pile was constructed of sixty pairs of disks of beet-root and radishes, separated by disks of wood; upon the upper part of each of which, by means of the border with which they were furnished, they poured acidulated tartrite of potash dissolved in vinegar; they afterwards placed at the lower extremity of the pile a leaf of cochlearia, and at its upper extremity a double band of gray paper soaked in vinegar. Every thing being thus disposed agreeably to Baronio's detailed description, frogs properly prepared were submitted to the action of the pile, by placing the leaf of cochlearia in communication with their spinal marrow, and the band of paper with their muscles. Three frogs successively subjected to repeated trials manifested no kind of movement: after having made without any success every trial capable of producing any effect, the pile was placed in communication with the electro-micrometer without any effect; the same instrument was afterwards presented to a pendulous pile constructed after M. Marechaux's method, composed of 60 pairs of new disks of copper and zinc, with the interposition of rounds of pasteboard not moistened. A tension was experienced of about 180°. At the same moment the frogs made use of in the experiments with the vegetable pile were placed in communication with the above pile, and they gave no signs of sensibility.

The Galvanic society, therefore, have not obtained the results indicated by Baronio; but their experiments served to convince them that the electro-micrometer, which they made

made use of in their experiments, is much better adapted than frogs for determining minute Galvanic effects.

XLII. *Description of a Check for Carriage Wheels on Rail-Roads. By Mr.* CHARLES LE CAAN, *of Llanelly, Carmarthenshire* *.

GENTLEMEN,

THE model of a rail-road waggon and check or stop, which I have this day the honour of presenting to you, I trust on examination will be found to possess the means of preventing those various accidents which have from time to time proved fatal to the horses employed in such service, particularly where the declivity is from twelve to sixteen inches to the chain, and the trade on such road principally descending.

The use of a horse employed on a rail-road is as frequently to check the velocity of a waggon or waggons loaded, (generally to the weight of two tons and a half each) so as that they may not exceed a certain degree of motion, as well as to draw them on such parts of the road as approach near upon or quite to a level. When the horse finds himself pressed upon beyond his power of resistance; to relieve himself, he is compelled to quicken his pace, by which means the velocity of the waggon exceeds any government during the continuance of the declivity which gave it such action: under such circumstances, the least trip of the horse terminates in a fall, by which, from the formation of a rail- or tram-road, the animal becomes injured notwithstanding every manual exertion. For the preservation of that valuable animal, and as a preventive to all such accidents in future, I turned my thoughts to the invention of the simple check or stop now before you, and which may be appropriated to carriages in general use.

As the utility of rail-roads daily increases in the opinion

* From *Transactions of the Society of Arts*, &c. 1806.—Ten guineas were voted to Mr. Le Caan for this invention, a model of which is preserved in the Society's Repository for the inspection of the public.

of the public, I trust every invention that may perfect such a system will add in some degree to its value, and aid in its advancement to perfection an object so desirable as the conveyance of every species of merchandise, and so requisite in a commercial country. I am, Gentlemen,

Your very obedient humble servant,

CHARLES LE CAAN.

Llanelly, Carmarthenshire,
April 11th, 1805.

To the Society of Arts, &c. Adelphi.

Reference to the Engraving of Mr. LE CAAN'S *Check or Stop for Carriages on Rail-Roads,* Plate VII. Fig. 1, 2.

Fig. 1.—A. A rail-road waggon.

B. the shafts in the direction as when drawn by a horse.

C C. the checks or stops made of oak, and shod with strong plate-iron; these checks should always be made somewhat thicker than the wheels.

D. a bolt and nut on which the stop C hangs: it is here fixed to the side of the cart, but it will be better for this bolt and nut to pass through the iron bar E, to which the shafts are connected, and the stop to hang from thence.

F. chains which keep the checks suspended whilst the horse is drawing, but at such a distance from the wheels, as to permit the checks to assume the position G, in Fig. 2, when the shafts are inclined as at H, in consequence of the horse falling from pressure or accident; in which case the waggon instantly stops, and prevents the horse from receiving any material injury, which the momentum of two or more waggons, arising from their velocity on roads upon an inclined plane, as I, Fig. 1, has unfortunately frequently occasioned.

It is necessary to observe, that to prevent the great trouble which would arise from turning the waggon round on a rail-road, it would be better to have a check to each of the four wheels; in which case, after the waggon has discharged its load at the place of its destination, the chains F F may be loosened from the shafts, and fastened upon hooks, one of which is shown at K, so as to keep the checks suspended above the road; the iron bar L, which attaches

attaches the shafts B to the body of the waggon, is then to be removed, and with the shafts to be placed in a similar manner at M on the other end of the waggon, which now becomes the fore part, the horse drawing it back to be again loaded. Whenever the waggon is ascending, the checks behind the waggon may occasionally be let down and used as rests to relieve the horse when necessary.

XLIII. *Experiments made upon new Ivory, fossil Ivory, and the Enamel of Teeth, in order to ascertain if these Substances contained Fluoric Acid. By Messrs.* FOURCROY *and* VAUQUELIN. *Read in the French Institute*.*

By a letter inserted in No. 165 of the *Annales de Chimie*, (see Philosophical Magazine, vol. xxiii. p. 264.) M. Gay Lussac, the pupil and friend of M. Berthollet, announces that Mr. Morichini, a chemist at Rome, had discovered the presence of the fluoric acid in new ivory, fossil ivory, and the enamel of teeth; he found that new ivory was almost entirely formed of fluate of lime, and that the enamel of teeth contained nearly 22 per cent. of fluate of lime.

This discovery is so interesting, that it becomes every man of science to ascertain the truth of it. We shall give in this paper the result of the experiments made by us upon the subject in the laboratory of the Museum of Natural History.

As it would have been difficult, and perhaps impossible, to make the sulphuric acid act conveniently upon these substances, if they had not been first cleared of their animal gluten, we began by calcining them in an open crucible.

1st, New ivory lost by this calcination -	45 per cent.
2d, Fossil ivory of Siberia - -	$41\frac{1}{2}$
3d, Fossil ivory of Loyo - -	41
4th, Fossil ivory of Lourque - -	18
5th, Fossil ivory of Peru, found at 1176 feet above the level of the sea - -	15
6th, Fossil ivory of Argenteuil -	14
7th, Enamel of teeth - -	$11\frac{1}{2}$

* From *Annales de Chimie*, tom. lvi. p. 37.

The

The differences which exist among the losses experienced by the different ivories in calcination may be explained by the state in which they are found: the fossil ivory of Siberia and of Loyo were almost entirely in their natural state; they preserved the greatest part of their animal gluten and their organization, while those of the canal of Lourque, of Peru, and of Argenteuil, had lost this matter and had become dry. The latter, also, easily split into scales, were extremely brittle, and sent forth only a very slight animal smell during calcination: as to the enamel of teeth, the small loss which it sustains by the same operation, announces that it contains much less humidity or animal mucilage than other bones, as was before well known: the latter assumed a bright blue colour upon being exposed to heat, which proves that it contains a notable quantity of phosphate of lime.

After having been calcined and pulverized, each of these matters was treated in the following manner, to know if we could ascertain the presence of the fluoric acid. Portions of the calcined ivories were placed in a common phial, and also in a small retort: four parts of concentrated sulphuric acid were poured upon the ivory; to these vessels was adapted a glass tube which was inserted in lime water, and they were then heated. In each of these experiments no more than twenty grammes were employed at a time, and never less than five.

Neither fresh ivory nor the enamel of teeth gave any traces whatever of the fluoric acid: the fossil ivories of Siberia and of Loyo were alike destitute of it; but those of the canal of Lourque, and Argenteuil, gave evident signs of this acid. In the latter cases, the upper part of the phials and retorts, as well as the tubes made use of, were covered with a white powder, the properties of which resembled silex; in the other cases nothing similar appeared.

These first results having inclined us to doubt the existence of the fluoric acid in new ivory, as well as in such ivory (although fossil) which still retained almost entirely, and without alteration, the animal matter, we made artificial mixtures, with fresh ivory and fluate of lime, in the several proportions of a 25th and a 40th part; and always, even

even in the latter case, we observed, in a very remarkable manner, the effects of the fluoric acid upon the glass when these mixtures were treated as above mentioned. These effects were even much more sensible than those produced by the fossil ivories of the canal of Lourque and of Argenteuil; which shows that the fluate of lime does not exist in a greater proportion than three or four per cent.

Those who have announced the discovery of the fluoric acid in ivory, do not assert that this substance was found ready formed in it, although they recollect that formerly Rouelle tried in vain to extract phosphorus from it; yet upon treating 300 grammes of it in the same manner in which bones are treated for the purpose of obtaining phosphorus from them, we obtained 15 grammes of very pure phosphorus in a very pure state. This quantity of phosphorus is nearly the same as that obtained from bones; and it is probable that we might have had still more, if the retort had not broken before the operation was entirely finished.

If by the first operation to which we submitted the fresh ivory we had not perceived any vestige of the fluoric acid, the circumstance which we are about to relate proves that it contains phosphoric acid in abundance, and probably as much as bones do.

The pungent smell which is disengaged at the moment when the sulphuric acid is mixed with calcined new ivory, cannot be regarded as a certain mark of the presence of the fluoric acid, because there is produced in this case a degree of heat so considerable as to volatilize with the water a small quantity of sulphuric acid: besides, this vapour manifests itself also during the mixture of the sulphuric acid with such bones, where the presence of the fluoric acid is not admitted.

Several chemists in Paris, having repeated the same experiments upon ivory, obtained results nearly similar to ours.

Although we have not found fluoric acid in new ivory nor in the enamel of teeth, as announced by M. Morichini, it is nevertheless clear, that such fossil ivories, of whatever country they are, as have lost their animal matter, contain some hundredths of fluoric acid. This truly singular circumstance seems to indicate that these substances are, after

cumstance

a long period, impregnated with fluoric acid,—a fact which supposes that this acid exists in the earth; for to suppose, with M. Klaproth, that the phosphoric acid is partly converted into fluoric acid, would be to admit an hypothesis too distant from our actual knowledge to appear even probable.

If the fluoric acid really existed in new ivory and the enamel of teeth, it must follow that a chemical analysis would find it in vegetable and animal substances; at least one would suppose that it would be developed in the living animal œconomy; which is extremely hypothetical, and without any rational foundation.

It seems more probable, therefore, that during the long continuance of these substances in the earth, they combine with the fluoric acid. By the former hypothesis the fluoric acid must be supposed to exist over all the world, since fossil ivories, wherever found, always contain this acid; by the second hypothesis we must be forced to admit the change of some principle in the ivory into fluoric acid, which is not altogether impossible: in truth, as we are ignorant of the nature of the fluoric acid, we cannot appreciate the manner or the cause of these transmutations in the present state of chemistry.

XLIV. *Proceedings of Learned Societies.*

ORIGINAL VACCINE POCK INSTITUTION,

Broad Street, Golden Square.

Dr. Shaw in the chair.—The chairman having stated that it was a subject of conversation, that one of the surgeons of this institution having inoculated his own child for the small-pox, was apparently inconsistent with the declarations to the public by this establishment, and requiring explanation; it was,—Ordered, that the following letter, with this notice, be sent to the gentleman in question.

O. V. P. J.——July 8, 1806.

Sir,

I am directed to transmit you the above notice, and to request the favour of an explanation; but the board and medical establishment beg you to understand, that they do not think they have a right to consider you as acting improperly,

properly, in employing variolous inoculation in your own family or elsewhere, except at the institution; because the great object here is to investigate the history of vaccine inoculation; and because reasons independent of your opinion of the two kinds of inoculation may have influenced your conduct. It is respectfully requested that you will favour the medical establishment with such an explanation as you think may meet with the affectionate regards of your colleagues. I have the honour to remain, &c.

John Doratt, Esq. WM. SANCHO, Sec.

Answer.

GENTLEMEN,

In answer to yours this day received, I beg leave to say, that I have not the smallest objection to state my reason for having inoculated my own child with the small-pox.

The child being near to the village of Streatham in Surry, had been exposed to the contagion of small-pox two days previous to my knowledge of the same; a child having been brought into the same room by accident, then in the fourth day of the eruption. In consequence of this unlucky circumstance, I considered it most prudent to inoculate him immediately with variolous matter. He went through the disease with much difficulty, and no inconsiderable degree of danger. Had the disease proved fatal, I should certainly have considered myself in a great measure to blame, for having neglected to vaccinate him at an earlier period, although he was only four months old when inoculated.

I am, Gentlemen, your most obedient,

Bruton Street, July 15, 1806. JOHN DORATT.

To the Weekly Board of the Vaccine Pock Institution.

Some of the governors asserting at this quarterly court, that the resolutions lately advertised * were neither strictly warrantable in the present state of facts, nor sufficiently explicit for the public,—the subsequent paper, by Dr. Pearson, was read at the weekly board, July 15th; and the author was requested to allow it to be printed for the members.

" Many families have become unsatisfied with the vaccine inoculation on two accounts; namely, the alleged in-

* See page 92 of the present volume.

stances

stances of the small-pox subsequently to the cow-pock, and the occurrence of disorders imputed to the new practice. Two opposite parties have for some time endeavoured to influence the conduct of the public; the one asserting and publishing numerous cases of failure and disease from the vaccine inoculation, the other disallowing such cases, but ascribing the occurrences to inattention, ignorance, imprudence, and misrepresentation; also to the human constitution being, in a small proportion of individuals, susceptible of the small-pox, and in course of the cow-pock, more than once. In this state of practice and opinions, it appears to be the duty of the medical establishment of this institution to communicate to the governors the information derived from their own experience on the two points in question of vaccine practice.

"I. *Concerning the power of the vaccine infection to render the human animal unsusceptible of the small-pox.*— Two instances only of alleged failure occurred during the years 1800, 1801, 1802, 1803, 1804, and 1805. I think, in both cases, the small-pox really took place after vaccine inoculation at the institution; but as it appeared from our register, that one of these patients, Songer Lemon, had not duly attended, there was a deficiency of evidence to prove that the cow-pock had been excited. In the other case, James M'Pherson's, it did appear from our records, that the patient had undergone the cow-pock, and it was admitted by the medical establishment, that the subsequent disorder was the small-pox. Having, however, up to the time of this last occurrence, an immense body of positive favourable evidence communicated from all quarters of the world, particularly by the correspondence of the institution with many practitioners in different parts of the united kingdom, in addition to the experience of our own establishment, we were rather inclined to suspect, in the case last mentioned, some error in our register, than to admit an exception to what appeared to be a law of the human animal œconomy. To appreciate justly our statements, it is necessary to explain that many of our vaccinated patients were, in the years 1800, 1801, and 1802, inoculated with variolous matter, and on good authority we know that many of them

were

were also exposed to the contact of persons ill of the small-pox. In the evidence of experience above represented, we should probably have confided without searching for further facts to prove the efficacy of vaccination, if besides the great number of asserted cases of small-pox subsequently to vaccine innoculation, many of which, on inquiry, were found to be utterly unfounded, and others at least unsubstantiated, there had not appeared from time to time a case so stated as to be undeniably the small-pox after the cow-pock. Accordingly, in the summer of 1804, the alarm was so great, especially from Mr. Goldson's temperate and judicious pamphlet, that the medical establishment were required by some of the governors to again institute the trials of re-inoculation. Sixty persons (chiefly those who had been vaccinated at the earliest periods of the institution) were inoculated in the most efficaciously known circumstances for the taking effect of variolous infection; viz. inoculation at the bed-side, and in contact with patients ill of the natural small-pox; inserting matter of the pock in the vesicle state; inoculating in an unusual number of punctured parts; of course always with recent and fluid matter; but without exciting, in a single instance, the small-pox. By those who have read our printed statement* of these trials, it may be recollected that the inquiry for subjects was the means of furnishing still more evidence than the cases reinoculated; for four out of five families to which children belonged who had been inoculated for the cow-pock, refused to submit to the test proposed; alleging that it was useless, as they had been exposed in vain to the influence of variolous effluvia after vaccine inoculation, by living with persons ill of the small-pox.

"A further considerable accession of evidence was obtained by the inquiry made on this occasion, whether or not any persons had taken the small-pox who had gone through the cow-pock in a general inoculation of three parishes, as early as 1799, under the direction of two members of our establishment. The result † was, that to the knowledge of the

* Statement of Evidence of the Original Vaccine Pock Institution, 8vo. Martin and Cuthell, 1804.

† See Statement of Evidence.

various

various practitioners living in those situations, no one * had taken the small-pox.

" It will be proper to bring forward another body of evidence different from the preceding. From its commencement this institution has been the appointed office for supplying weekly the army, the navy, and ordnance department with vaccine matter; yet this constant intercourse has not afforded any authentic cases of failure which can be recollected—as far as known, the reported ones appeared on investigation to be either misrepresentations or could not be proved.

" It must not be concealed, however, that from time to time cases were made known, in the practice elsewhere, of the occurrence of small-pox subsequently to the cow-pock; which, according to the evidence, we could not justly reject, although conviction was not always felt that the case was one of real small-pox after the authentic vaccina.

" In the mean time numerous trials were made at this institution of opposing variolous and vaccine matter to each other in various situations, which demonstrate the anti-variolous power of the latter. These experiments cannot be here detailed, but they are recorded in our minute-book as materials for continuing the investigation of the laws † of agency of vaccine matter.

" At this institution, by a great number of trials, two laws, first asserted in 1799, seem to have been fully confirmed, which may be considered as demonstrative of the power of the vaccine affection, if duly excited, to induce unsusceptibility of the small-pox. The laws alluded to are, 1. That the human constitution is incapable of undergoing the vaccine affection a second time, if by the first it has been rendered unsusceptible of the small-pox. 2. That the human constitution is incapable of undergoing the vaccine

* A failure has been lately reported to the writer of this paper, but there has been no opportunity to ascertain the fact by legal evidence.

† The Original Vaccine Pock Institution has published one Report of the progress of this investigation, which with the proposed subsequent ones, probably, will ultimately establish principles for secure practice. See " The Report on the Cow-Pock Inoculation, &c. during 1800, 1801-2, &c. by the Physicians to the Institution," 8vo. 1803.—Reynell.

infection

infection at all, if it has been rendered incapable of undergoing the small-pox a second time, by having already gone through this disease. Hence the truly important practical conclusion,—That inoculation with vaccine matter affords an equally just criterion of the state of unsusceptibility of either small-pox or the cow-pock, subsequent to either of these affections.

"On the grounds of the above different sources of evidence, the opinion of the medical establishment in general seemed to incline to that of the asserted failures after vaccination being either imputable to certain circumstances not being attended to, which might have shown that the patients were not duly vaccinated; or else that the subsequent disorder was not substantiated to be really the small-pox. But the occurrence in May last, of two failures in the practice at this institution, compelled us to own that persons who have gone through the cow-pock with all the appearances, and in the course deemed, as we apprehend, by competent judges, to be those of the cow-pock, and which are alleged by them to give security—I say, that after such an affection the small-pox may take place in a small proportion of the number vaccinated. These two adverse occurrences unfortunately rendered our opinion of a former case of small-pox after cow-pock no longer tenable; namely, that an error had probably crept into our register of the case of M'Pherson. As it does not appear that the three above failures were owing to any known measures or circumstances, and as the inoculations were in circumstances supposed adequate for obtaining security, we must conclude, that according to our experience, vaccine inoculation does not in all cases produce the unsusceptibility desired. It is, however, abundantly proved by the preceding evidence, that the proportion of insecure cases is very small. Every estimate must be on grounds merely conjectural; but on such grounds it may be stated to be one out of a thousand. However, it ought to be also stated, that this proportion is on the supposition that all persons left susceptible of the small-pox after cow-pock have taken the former disease, which is obviously hitherto undecided. These being truths, false conclusions, as it now appears, have been drawn, viz. 1. That the inocula-

 tion

tion for the cow-pock is equally efficacious to that for the small-pox. 2. That the failures are in every instance imputable to unskilfulness, ignorance, or inattention. It is however easy to conceive, that in places of small population, or in places where the small-pox is neither epidemical, nor where persons inoculated for that disorder are allowed to live with children who have been vaccinated, or to have intercourse with society at large, the natural small-pox may not appear for many years, and with some precautions may be, perhaps, extinguished. In such circumstances many lives may be saved. In London, the population being so considerable—persons having greater liberty to act according to their own opinion—and having been much influenced by writers, opposers to the cow-pock inoculation—also the variola having been highly epidemical, it is, I think, obvious that such circumstances must excite the small-pox in a large proportion of susceptible persons. This is what happened in the year 1805, and it is perhaps fortunate for the public that the variolous infection was disseminated, as but for that circumstance the state of insecurity might not have been made appear. The two cases which have induced a change in our opinion were those of Anne Maber, No. 33, and Mary Maber, No. 34, on Dr. Nihell's list, who, as appears from the register, underwent the regular cow-pock in the year 1800. In May 1806, living in the same room with a child in the inoculated small-pox, they took the small-pox, as allowed by the members at the board, before whom they were examined. The medical establishment were now convinced that many of the cases published, as cases of the small-pox after the cow-pock, were really such cases, although not legally proved; nor does it appear that any known appearances or circumstances would have indicated the insecurity in our patients, the two Mabers. Accordingly, it would not be just conduct to withhold from the public, that at present no one can be authenticated to be secure from the small-pox by vaccine inoculation, unless the test be employed of reinoculation. Confiding in our experience, the vaccine matter is equally to be relied upon as the variolous, to afford the required proof; but those who may not be acquainted with our experience will probably not be satis-

fied,

fied, unless with variolous matter. But to determine a most valuable fact, it is advisable to insert vaccine matter in one arm and variolous in the other. No one would object to this proposal, if the harmlessness of repeated insertion of matter, fully ascertained at this institution, were commonly known. By this mode of procedure, a double test is applied, and at the same time it will be shown that the state or disposition to be affected, or not, is alike to both kinds of matter.

" It is designed to relate our own experience; and that has not informed us that there is any such thing as spurious matter in any proper sense of the term, *i. e.* a sort of matter which produces an affection mistaken for the cow-pock, by successive insertions of it from subject to subject. Matter which produces an anomalous pock, either, on the succeeding insertions, excites the regular cow-pock, or fails to excite a similar anomalous pock: this is, I say, according to our experience. And in the cases of such anomalous pocks, sometimes we have found the unsusceptibility of the small-pox produced, and at other times not; for in such cases we always insisted upon reinoculation.

" II. Concerning the other supposed fact, which has with many persons created an unfavourable opinion of vaccine inoculation; namely, the producing various disorders, even of a new kind, we must have longer experience to arrive at a determination. The most experienced we are persuaded will accede to this proposal :—If, on some occasions, seemingly new forms of eruptive disorders appear, let it be remembered that they do so to accurate and extensive observers, not only in persons who have, but in those who have not had the cow-pock. However, there does appear probable evidence that certain slight eruptive complaints are the peculiar offspring of vaccination; but that the formidable diseases represented by some authors are the peculiar effects of it, we must demur either to allow, or disallow, for want of more experience. In the mean time we can safely attest, that on the whole, much less frequently disorders supervene to the cow-pock than to the small-pox inoculation. Yet it is our duty to declare, that on good authority in the course of the eight years practice of vaccination, four

or five fatal cases have occurred from the affection of the part inoculated; although at the institution, and in our private practice, we have had no such cases; and that sore arms much less frequently occur than from variolous inoculation is quite evident.

"In conclusion: We find in our experience, both private and public, the vaccine affection a much less painful one than the variolous inoculation, and in no instance * fatal—that it is not propagated by mere contact, or effluvia—that it is never epidemical—that it produces unsusceptibility of the small-pox if well conducted, in perhaps 499 out of 500 cases—that by modes, hereafter to be explained, there is good ground for believing that the practice may be so conducted as to make the proportion of failures much less—that the advantages are on many accounts so great as to amply compensate for the small sacrifice and trouble of a second inoculation—but lastly, it ought to be constantly in view, that the security against the small-pox cannot be absolutely obtained unless inoculation be re-instituted; and hence, that for the future it does not seem justifiable to neglect to propose the second † inoculation in every instance; and it is further our duty to propose it, as a security for the past, also to those who have been already vaccinated ‡."

* In a case communicated, an infant died on the 10th day after inoculation, with convulsive fits.—See Minute Book.

† The second inoculation may be instituted either in four days or any longer time after the first inoculation has taken effect. See Med. and Chirurgical Review, vol. 12.—Med. and Surgical Journal of Edin. vol. 1.—Tilloch's Philosophical Magazine, vol. 23.

‡ Although at this time the appearances or circumstances are unknown, during the vaccine affection, which necessarily give security, and hence re-inoculation must be provisionally resorted to; yet, it is reasonable to expect that hereafter the subject may be so far investigated as to determine the question by one inoculation.

Note.—It has been urged, that the evidence in this paper, at most, only proves that vaccination destroys the susceptibility of the small-pox for a limited time: and hence that the second inoculation only affords a proof of the time for which that unsusceptibility subsists. This may be, I think, justly redargued. 1st. The failures afford only equivocal evidence of the alleged temporary unsusceptibility. 2d. Such a temporary state is against analogy. 3d. There is no inconsiderable positive evidence of permanent unsusceptibility.

The

The following Resolutions were next read, and unanimously agreed to be published:

I. That it does appear, on adequate evidence, that persons who have gone through the cow-pock, in the manner commonly believed to give security, have in the proportion of one in a thousand subsequently taken the small-pox, according to the experience of this institution up to the present time.

II. That disorders which can be at all reasonably imputed to inoculation have been observed less frequently after the vaccine than after the variolous inoculation.

III. That considering the slightness of the cow-pock, and that no fatal case has occurred in the practice of this institution, vaccination is greatly preferable to variolation; and especially on account of its not being propagated in any way but by inoculation, it is infinitely more valuable to the public.

IV. That it be recommended to every one who has been, or shall be, vaccinated, to be re-inoculated with variolous or vaccine matter.

V. That according to the experience of this institution, the test of the insertion of vaccine matter (which is perfectly harmless) is to be relied upon equally with that of the variolous.

VI. That there are good reasons for imputing the late mortality by the small-pox to its having been highly epidemical last year, as well as to the influence by which some writers, opposers of the cow-pock, were enabled to prevail upon a great number of persons to be inoculated for the small-pox; thus multiplying the sources of variolous infection.

Our readers in general may at first be a little surprised, and the sanguine advocates of vaccination be mortified on perusing the above declarations of an independent society; but after considering the subject, it will appear, that still the cow-pock inoculation possesses all the advantages ever proposed by its most reasonable advocates, except that in a small pro-

portion of cases the inoculated persons are still liable to the small-pox. To guard against accidents in the course of life, re-inoculation is recommended. The only question now is, whether vaccine inoculation will be preferred to the variolous subject to a second insertion of variolous or vaccine matter. At any rate, the institution has discharged its duty in declaring the risque incurred of not testing subjects after the cow-pock inoculation, so that no reproach can justly be offered in the event of any vaccinated person in the course of life being seized with the small-pox.

SOCIETY OF ARTS AND SCIENCES OF GRENOBLE.

This society has offered a prize of 500 franks for the best treatise upon the Mineralogy of the Canton of Loysans in the department of Isere.

TEYLERIAN SOCIETY AT HAARLEM.

The following is announced as a prize question by the above society: "What do we know historically of the alterations which the earth has undergone in some districts in consequence of the flood, and of the variety of causes which occasioned these alterations?"

The answer must be sent in to the society on or before the 1st April 1807, and the prize is a gold medal of the value of 400 Dutch guilders.

THE ROYAL BOHEMIAN SOCIETY OF LEARNING.

This society having as yet received no satisfactory answer to the prize question announced by them in April 1804, have again offered a prize of 700 golden ducats for the best answer to the following question: "By what method can the various adulterations of the different necessaries of life be best ascertained or lessened, by radical examination, or otherwise?"

The answers must be sent to the director of the Royal Bohemian Society at Prague, on or before the 1st of June 1807.

XLV. *In-*

XLV. *Intelligence and Miscellaneous Articles.*

RUSSIAN VOYAGE OF DISCOVERY.

Extract of a Letter from Captain Krusenstern to the Academician Schubelt. Dated Peter and St. Paul's Harbour, June 8, 1805.

We have now happily terminated our voyage to Japan; the day before yesterday I anchored here. Upon the 7th of September 1804, we sailed from Kamtschatka. The lateness of the season induced me to think of nothing else than landing the ambassador as soon as possible in Nangasacki, and at last we descried the coast of Japan. A violent storm drove us off the shore. We regained it after its fury had abated; but as we were approaching the land we were again assailed by a furious whirlwind when close in with the shore. A miracle alone saved our lives, which was a sudden alteration of the wind. I sailed through Van Diemen's Straits, because they are so differently laid down in the English and French charts, both of which we found equally faulty. These Straits I caused to be laid down in our charts with such accuracy that nothing more remains to be done on that subject. We measured at least 1000 angles. We found five islands in Van Diemen's Straits; and it is scarcely credible how inaccurate all the charts of this part of Japan are. Upon the 8th of December we anchored in Nangasacki, where we remained until the 18th of April, 1805. I enjoyed great repose for these seven months, and did not misspend my time. To ascertain the longitude of Nangasacki by the moon's distance was the first object of myself and Dr. Horner. Each of us measured more than 500 distances. The situation of Nangasacki, which has been known for these 200 years, was ascertained by us to be exactly 230° 8′ west from Greenwich, the latitude 32° 44′ 50″. The months of October, November, and December were very agreeable; the weather was mild, and never stormy. With January the winter commenced: the thermometer fell often to the freezing point; it snowed also at intervals, but

 very

very slightly. The Japanese gave me every assistance in preparing the new plan of the harbour of Nangasacki. Our voyage from the entrance to the inner part of the harbour lasted three months, and we halted five different times in the course of our survey. These relaxations from our labours were of great use to us, and very pleasant. Lieutenant Lowenstern has made a valuable collection of drawings of all the vessels we saw in this harbour, with all their various colours and decorations, which every man of rank carries according to his degree. Baron Billingshausen has prepared several models of Japanese boats, and a very correct drawing of a Chinese junk. Counsellor Tilesius has made a rich collection of beautiful drawings of birds, fishes, and marine productions, &c., and Dr. Langsdorf has stuffed and prepared the most of these birds, fishes, &c.

I have already acquainted you from Kamtschatka of my plan, which is to produce a correct chart of the Great Ocean, for which I have been collecting materials for these several years. This idea was strengthened at Nangasacki, where I had both time and opportunity: I have it at present in contemplation to write a particular work upon the Great or Pacific Ocean. Upon my return to Europe, when I enjoy more leisure, I shall proceed in that work.

As soon as we received our dispatches from Jehdo, we laboured night and day to get away. On the 17th of April, the ambassador came on board; the Japanese towed us from our anchorage out into the open sea, although there was every presage of a storm, which indeed soon followed. I weighed anchor on the 18th of April, and put to sea. We were very fortunate in enjoying good weather during our examination of the coasts of Japan. As I was obliged to be in Kamtschatka by the beginning of June, we could lay down but a very few points in Japan. The rocks which surround a little island near Cape Patience were the last part we saw of Sagalia: immense islands of ice drove us to the south-east. We then resolved to sail for Kamtschatka without delay, in order to put ashore the ambassador, who wished to travel towards St. Petersburg. At last I have anchored

chored in St. Peter and St. Paul's harbour. The chamberlain Resanow goes towards Kadiak. The above is a short account of our voyage for the last nine months *.

MINERALOGY OF HANOVER.

When the French government took possession of Hanover, M. Heron de Villefosse, a celebrated mineralogist, was sent from Paris to the mines of Hartz in that electorate, for the purpose of watching over the progress of these works, and to protect them as much as possible from the accidents of war. The success of his mission has surpassed the expectations of his government; and he has transmitted to the minister of the interior some very interesting details upon the subject of these mines, accompanied with descriptions of the various processes in use, both in the mines below ground, and in the forges and workshops after the ore is brought out. Among the treatises transmitted by M. Heron de Villefosse, the most interesting are upon the melting of the minerals of lead and silver, a survey of the mountains and mines of all Hartz, and a very fine mineralogical chart. The aspect of the country is rugged and picturesque, the air sharp, and the temperature extremely variable. The winters there are always long, and cold. No grain of any kind is cultivated in that district; but it produces a great number of cattle. The people are robust and healthy, and they are strongly attached to their native mountains.

The population of this district was estimated in 1804 at 22,282 inhabitants. Clausthal is the name of the principal town in the Hartz, and its population is 7,622. The mines of lead, silver, and copper are divided into three districts; and the machinery by which they are wrought is reported to be very curious.

* Before proceeding on his voyage, Captain Krusenstern caused all the water-casks intended for the supply of the crew to be charred in the inside, a precaution which he found answer the purpose of preserving the water sweet during the whole voyage; in the French journals, the merit of this discovery of preserving water during long voyages, is ascribed to M. Berthollet. This expedition lately returned to Europe, stopped for a few days at Portsmouth, and then proceeded for Russia.

MUNGO PARK.

We are happy to state, that the accounts lately circulated of the death of this traveller are believed to be erroneous. No such account has reached government, nor have there been any late arrivals from that country by which the information could have been received. It is the opinion of Sir Joseph Banks and other gentlemen personally acquainted with Mr. Park, and deeply interested in the success of his mission, that it is the old report of the death of part of the soldiers and carpenters newly revived and greatly exaggerated.

AEROSTATION.

The misfortune which lately befel M. Mosment, at Lisle, has not put a stop to the rage for aërial exhibitions on the Continent. In the months of May and June last, Dr. Jungius, of Berlin, made several ascensions in the neighbourhood of that city, with complete success. He is the first German aëronaut.

Madame Blanchard, the wife of the aëronaut of that name, ascended from Bourdeaux in the beginning of May last, and descended a few leagues from the place of ascension, without any accident. Her husband is at present involved in a dispute with his brother-aëronaut Garnerin, on the subject of the proper requisites of a balloon. M. Blanchard maintains, in contradiction to M. Garnerin, that balloons in the constructing or ornamenting of which any metallic body is made use of, run no more risk than any others upon meeting with electrical meteors. M. Blanchard says, that he has often passed through electrical clouds in gilt balloons without meeting with any accident.—Much may be said on both sides of this argument.

MISCELLANEOUS.

On Wednesday, the 22d of April, a very uncommon phænomenon was observed at Arbroath, between the hours of two and four P.M. A bright halo appeared round the sun of a very uncommon magnitude, the diameter being 54°: also a large luminous circle parallel to the horizon, diameter about 98°, its altitude equal that of the sun. Its centre nearly

nearly in the zenith of Arbroath. The periphery of this circle passed directly through the centre of the sun: on the eastern part of this circle two beautiful parhelia were distinctly visible, as also two much more resplendent on the western part.

Professor Aldini of Bologna, the author of several works upon Galvanism, was introduced as a member of the new academy at Turin, by the princess Elizabeth.

Lalande, the astronomer, has presented to the emperor Napoleon, his History of Astronomy for the year 1805.

M. Laurent, of Paris, has invented a flute of flint-glass, which for the fineness of its tone far surpasses those of wood. The Imperial Conservatory of Paris have examined it, and declared themselves highly pleased with the invention.

M. Escher, so well known on account of his lucubrations in Bergman's Journal, is at present engaged on a geological tour through the Tyrol and the Western Alps between Dauphiny and Savoy.

Abbé Stasic, celebrated for his extensive acquaintance with Polish literature, has made a present of 5000 ducats to the Society of the Friends of Literature at Warsaw, for the purpose of enabling them to erect a hall, &c. in which to hold their sittings.

M. Meyer, a celebrated landscape-painter of Germany, has published the prospectus of a new work, under the title of "Views on the Rhine." They are to be engraved after the best English manner: and areto be thirteen inches by eighteen, each, in size, accompanied with descriptive letter-press; one of these engravings will appear periodically every three months.

MEDICAL REFORM.

A most important scheme of this description is in contemplation. No one who is at all an observer of what is passing in society can be ignorant of the abuses committed, and impositions practised, by persons of inadequate or no education assuming the rank of those who have been duly educated; depriving them of those emoluments to which they are entitled, as well as injuring society by their ignorance of medical science. Dr. Harrison of Horncastle

Horncastle is the prime mover, supported by many physicians and surgeons of the highest character in London. In this undertaking, among other men of rank (not of the medical profession) the right honourable sir Joseph Banks benevolently countenances and assists in the attempt to effect a new arrangement. Several meetings have been held at his house in Soho Square, and some progress has been made in preparing the business to be brought before parliament, with the assistance of the right honourable chancellor of the exchequer, lord Henry Petty.

LECTURES.

At the Medical Theatre, St. Bartholomew's Hospital, the following Courses of Lectures will be delivered during the ensuing winter:

On the Theory and Practice of Medicine, by Dr. Roberts and Dr. Powell.

On Anatomy and Physiology, by Mr. Abernethy.

On the Theory and Practice of Surgery, by Mr. Abernethy.

On Comparative Anatomy, and the Laws of Organic Existence, by Mr. Macartney.

On Chemistry, by Dr. Edwards.

On Midwifery, and the Diseases of Women and Children, by Dr. Thynne.

The Anatomical Demonstrations and practical Anatomy, by Mr. Lawrence.

The Anatomical Lectures will begin on Wednesday, October the 1st, at two o'clock; and the other Lectures in the course of the same week. Further particulars may be known by applying to Mr. Nicholson, at the apothecary's shop, St. Bartholomew's hospital.

At St. George's Hospital, and Great George Street Hanover Square, in the first week of October will commence a Course of Lectures on the Practice of Physic, Therapeutics, and Chemistry, in the lecture-room No. 9, Great George Street Hanover Square, (removed from Leicester Square) at the usual morning hours, viz. the Medical Lecture at eight, and

and the Chemical at a quarter after nine, by George Pearson, M. D. F. R. S. Senior Physician of St. George's Hospital, of the College of Physicians, &c.

A register is kept of Dr. Pearson's patients in St. George's hospital, and an account is given of them at a Clinical lecture every Saturday morning at nine o'clock.

Proposals may be had at St. George's Hospital, or at No. 9, George Street Hanover Square.

At the Theatre of Anatomy, Blenheim Street, Great Marlborough Street, the autumnal course of Lectures on Anatomy, Physiology, and Surgery, will commence on Wednesday, October 1, at two o'clock in the afternoon, by Mr. Brookes.

In these lectures the structure of the human body will be demonstrated on recent subjects, and further illustrated by preparations, and the functions of the different organs will be explained.

The surgical operations are performed, and every part of surgery so elucidated, as may best tend to complete the operating surgeon.

The art of injecting, and of making anatomical preparations, will be taught practically.

Gentlemen zealous in the pursuit of zoölogy will meet with uncommon opportunities of prosecuting their researches in comparative anatomy.

Surgeons in the army and navy may be assisted in renewing their anatomical knowledge, and every possible attention will be paid to their accommodation as well as instruction.

Anatomical converzationes will be held weekly, when the different subjects treated of will be discussed familiarly, and the students' views forwarded.—To these none but pupils can be admitted.

Spacious apartments, thoroughly ventilated, and replete with every convenience, will be open at five o'clock in the morning, for the purposes of dissecting and injecting, where Mr. Brookes attends to direct the students, and demonstrate the various parts as they appear on dissection.

An extensive museum, containing preparations illustrative of every part of the human body, and its diseases, apper-

tains

tains to this theatre, to which students will have occasional admittance. Gentlemen inclined to support this school by contributing preternatural or morbid parts, subjects in natural history, &c. (individually of little value to the possessors,) may have the pleasure of seeing them preserved, arranged, and registered, with the names of the donors.

The inconveniencies usually attending anatomical investigations, are counteracted by an antiseptic process, the result of experiments made by Mr. Brookes on human subjects at Paris, in the year 1782; the account of which was delivered to the Royal Society, and read on the 17th of June, 1784. This method has since been so far improved, that the florid colour of the muscles is preserved, and even heightened.—Pupils may be accommodated in the house.—Gentlemen established in practice, desirous of renewing their anatomical knowledge, may be accommodated with an apartment to dissect in privately.

In the second week of September, Mr. Thelwall will commence, at his house in Bedford Place, Russell Square, a series of Evening Lectures, Literary, Critical and Miscellaneous: The lectures will be delivered on Mondays, Wednesdays, and Fridays, and will be accompanied with criticisms on the elocution of theatrical and other public speakers. And on Wednesday, October 1st, Mr. Thelwall will commence his regular course on the Science and Practice of Elocution: Mr. T. likewise purposes to deliver Morning Lectures to select classes, who may be desirous of entering more minutely into the profound and critical parts of the science; and also, to continue his private instructions to persons afflicted with impediments of speech; or desirous of improvement in the various branches of elocutionary accomplishment, and the graces of English composition, &c.

LIST OF PATENTS FOR NEW INVENTIONS,

For the Month of August 1806.

A grant to Richard Tomkinson, of the town of Liverpool, in the county of Lancaster, salt merchant; for his entire new machine, engine, or instrument for making white salt, and preparing brine to make white salt. Dated Aug. 1.

To

To James Rawlinson, of the town of Derby, gent.; for his certain improvements or apparatuses, commonly made use of as trusses, or bandages for ruptures. Dated August 1.

To Peter Marsland, of Heaton Norris, in the county of Lancaster, cotton spinner; for his improved method of weaving cotton, linen, woollen, worsted and mohair, and each or any of them, by machinery. Dated August 1.

To Thomas Fricker, of New-Bond Street, in the county of Middlesex, paper-hanger; and Richard Clarke, of Manor Street, Chelsea, in the said county, paper hanging manufacturer; for their new mode of decorating the walls of apartments, in imitation of fine cloth: without joint, seam, or shade, by means of cementing of flock on walls of plaster, wood, linen or paper. Dated August 1.

To Ralph Walker, of Blackwall, in the county of Middlesex, engineer; for his improved mode of making ropes, and cordage of every dimension or size, by not only making all the yarns bear equally in the strand, and laying the strands uniformly in the rope, but also, by making the rope or cordage from the yarns in the same operation. Dated August 9.

To Josias Robbins, of Liverpool, in the county of Lancaster, mill-wright; and James Curtis, of the city of Bristol, coppersmith; for certain improvements in boilers for manufacturing sugar, and in the mode of fixing the same, whereby much labour and fuel will be saved. Dated August 20.

To John Bywater, of the town and county of the town of Nottingham; for his improvement in certain sails of ships, and other navigable vessels, and the mode of working the same. Dated August 22.

To John Curr, of Belle Vue house, in the county of York, gent.; for his method of laying and twisting the yarns that compose a rope, by which method the yarns of a rope have a better and more equal bearing than they have in a rope made in the common way. Dated August 23.

METEORO-

METEOROLOGICAL TABLE,
By Mr. Carey, of the Strand,
For August 1806.

Days of the Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
July 27	60°	72°	68°	29·64	45°	Fair
28	59	71	60	·57	31	Showery
29	62	69	59	·68	0	Showery
30	63	69	61	·82	0	Showery
31	63	72	60	·71	56	Fair
Aug. 1	60	66	58	·69	25	Cloudy
2	60	70	57	·52	50	Stormy
3	60	69	59	·80	48	Showery
4	58	68	61	·95	48	Fair
5	59	71	60	30·00	53	Fair
6	58	68	60	·00	57	Cloudy
7	64	74	64	29·98	54	Fair
8	66	75	64	·92	51	Fair
9	67	77	66	·89	55	Fair
10	66	76	64	·99	55	Fair
11	65	73	67	30·00	57	Fair
12	63	73	60	29·92	63	Fair
13	60	67	56	·71	5	Showery
14	57	68	54	·78	10	Showery
15	55	67	56	·92	66	Fair
16	57	71	60	30·19	56	Fair
17	60	73	64	·20	62	Fair
18	64	72	64	·10	54	Fair
19	66	72	67	29·98	47	Fair
20	66	72	66	·78	25	Showery, with thunder
21	65	70	65	·69	21	Showery
22	64	74	64	·82	59	Fair
23	65	73	64	·95	54	Fair
24	65	74	57	30·09	52	Fair
25	57	70	59	·10	52	Fair
26	57	64	57	29·47	0	Rain

N. B. The barometer's height is taken at noon.

Errata, Vol. xxiv.—Page 360, line 20, for ·00421, read 0·0421; line 21, for ·00463, read 0·0463; line 23, for ·00442 will give ·00394, read 0·0442 will give 0·0394,—*dele* in the same page the last two lines, viz. Sulphate of lime, and 20·4: page 361, for "Brought forward 20·4," read *sulphate of lime* 18 grains; line 6, for ·00394, read 0·0394; line 7, read 29·6394; in the next line read, loss 0·3606.

XLVI. *Extract from* M. KLAPROTH's *Memoir upon the Sulphuric Acid. Read at the Philomathic Society of Berlin.*

THIS memoir has for its object to determine the respective quantities of the elements of the sulphuric acid, and of sulphate of barytes. It details the analyses made of these substances by the following different chemists.

According to Lavoisier, the sulphuric acid is formed of 0·69 of sulphur and 0·31 of oxygen.

According to Berthollet, it is formed of 0·72 of sulphur and 0·28 of oxygen.

M. Thenard gives 55·56 of sulphur and 44·44 of oxygen, as its component parts.

According to Chenevix, it is formed of 51·50 of sulphur and 38·50 of oxygen.

According to Tromsdorff, it contains 70·00 of sulphur and 30·00 of oxygen.

According to Richter, it contains 42·05 of sulphur and 57·95 of oxygen.

Finally, according to M. Bucholz, it is formed of 42·05 of sulphur and 57·05 of oxygen.

From the above results, it is evident that the two last analyses, although made in a different manner from each other, have the strongest resemblance, and consequently are best entitled to confidence.

M. Klaproth, however, is of opinion that he ascertained, by his own experiments, the respective quantities of the elements of sulphuric acid, and which he made that he might apply the results afterwards with more certainty to the analyses of pyrites or metallic sulphurets. In order to attain this object, he employed, like his predecessors, the nitric acid and carbonated barytes: the proportion of the elements of the latter he had previously determined to be 78 of barytes and 22 of carbonic acid.

But M. Bucholz has since asserted that there are in this salt 79 of barytes and 21 of carbonic acid. M. Klaproth has therefore repeated the analysis with every precaution, and has again obtained the same results as before. M. Bucholz also only admits of 25 of carbonic acid in strontian, while M. Klaproth found 30 in all the analyses he made of this substance.

The results obtained by the analyses of sulphate of barytes, as made by various chemists, are not less at variance.

According to Fourcroy, it is composed of 66·00 of barytes and 34·00 of sulphuric acid.

Messrs. Clement and Desormes give 67·82 of barytes and 32·18 of sulphuric acid as its component parts.

 M. Thenard

M. Thenard makes it to consist of 74·82 of barytes and 25·18 of sulphuric acid.

M. Chenevix says it contains 76·50 of barytes and 23·50 of sulphuric acid.

M. Kirwan gives 67·00 of barytes and 33·00 of sulphuric acid as its proportions.

M. Richter gives 69·00 of barytes and 31·00 of sulphuric acid.

And, finally, M. Bucholz says it is composed of 67·00 of barytes and 33·00 of sulphuric acid.

Thus we see that if we except the analyses of Messrs. Chenevix and Thenard, all the rest have a great resemblance to each other; and if we take the medium, that of Kirwan is the truest, which induced M. Klaproth to adopt it.

It is here necessary to explain the means employed by M. Klaproth in his researches: he introduced into a retort 200 grains of pure sulphur, and eight ounces of pure nitric acid, of the specific gravity of 1·320. He distilled until about three fourths had passed into the receiver. What came over was poured back into the retort, and was again distilled; he afterwards added eight ounces of acid, and distilled the whole.

M. Klaproth found that the unburnt sulphur weighed 48 grains and a half; he therefore had 151 converted into sulphuric acid.

The produce diluted in a certain quantity of water was mixed with muriate of barytes, until there was no longer any precipitate produced in the liquor. The sulphate of barytes well washed and dried weighed 1109 grains, but when calcined in a platina crucible, its weight was reduced to 1082 grains.

In order to ascertain the proportion of the constituent principles of the concrete sulphuric acid, M. Klaproth took 100 grains of very strongly concentrated sulphuric acid, of the specific gravity of 1·850; he diluted in in 15 parts of water, and poured in muriate of barytes until no more precipitate was formed. The sulphate of barytes carefully washed and dried weighed 225 grains. From these facts it results,

1st, That 100 parts of sulphuric acid of the specific gravity of 1·850, are composed of 74·04 of concrete acid and 25·06 of water; or, of 31·05 of sulphur, 42·09 of oxygen, and 25·06 of water.

2d, That 100 parts of concrete acid are formed of 42·03 of sulphur and 57·07 of oxygen.

3d, That 100 parts of calcined sulphate of barytes contain of barytes 67·00, sulphur 14·00, and oxygen 19·00.

XLVII. *Prin-*

XLVII. *Principles of the Science of Tuning Instruments with Fixed Tones.* *By* CHARLES EARL STANHOPE.†

SEVERAL of the first mathematicians, as well as many of the most distinguished musicians, have spent much time in endeavouring to discover the best manner of tuning instruments with fixed tones; but their efforts have not, as yet, been attended with the desired success.

When I began this inquiry, I had the curiosity to converse with sixteen or eighteen of the most eminent musicians in England upon this subject. Half of them did then approve of what is called THE EQUAL TEMPERAMENT. This term will be explained hereafter. The other half, on the contrary, reprobated that mode of tuning, as never satisfying the ear perfectly in any one key whatsoever.

A science is evidently in a very imperfect state, when the first proficients in that science not only differ, but even hold decided opinions diametrically opposite to each other. I determined, therefore, to make a careful and methodical investigation, and I can now communicate with particular satisfaction the result to the public.

An ingenious and useful tuning instrument, called a monochord, has long since been invented, by means of which the relation between the lengths of strings or wires which produce different musical sounds can be accurately ascertained.

When a string or wire of uniform thickness is *reduced* in length, it yields a *sharper* sound, provided that the tension of the wire be not altered.

If a string or wire of uniform thickness, stretched on a well constructed monochord, be reduced to one half of its original length; then, that one half will yield the sound of that higher note which is scientifically termed the *perfect octave*, provided that the tension of the wire be not altered. If, for example, the length of an uniform wire be thirty inches, and be so stretched as to yield the same sound as that note, for instance, which is commonly termed the first bass C; then, in order for that same wire, under the same tension, to give the exact sound of the C next above, which is termed the middle C, the length of the

† This interesting treatise by EARL STANHOPE has been printed in *Stereotype;* and having been favoured by his Lordship with the use of the plates, we are enabled to lay it before our readers entire, and in the shape in which it originally appeared. This circumstance accounts for the want of uniformity in the margins of our present Number; which, however, is more than compensated by the accuracy of the tables, which might otherwise have been less perfect. We have farther to remark, that these pages have not been worked off at our common press. Mr. WILSON, of the Stereotype Office, Duke Street, Lincoln's Inn Fields, printed them for us at the iron press of the *second* construction, invented by EARL STANHOPE, and manufactured by Mr. ROBERT WALKER, of Dean Street, Oxford Street.

wire must be reduced to fifteen inches. The *perfect octave* is generally called the *octave*, for the sake of conciseness.

If the wire be reduced, not to one half, but to two thirds of its original length, namely, in the supposed case, to twenty inches; then, the sound produced by those two thirds will be that sound which is termed a *perfect fifth*, or a *perfect quint*.

If the wire be reduced to three fourths of its original length, namely, in the supposed case, to twenty two inches and a half; those three fourths will yield the sound which is termed a *perfect fourth*.

And if the wire be reduced to four fifths of its original length, namely, in the supposed case, to twenty four inches; those four fifths will yield the sound which is termed a *perfect third* †.

In like manner, any pitch whatsoever within the compass of the monochord may be obtained, by regulating the length of the wire to the exact degree that is requisite for that purpose.

Having stated precisely what I mean by the expressions *perfect octave*, *perfect quint*, *perfect fourth*, and *perfect third*, I will now shew that there is, in every musical instrument which has exactly twelve fixed keys, or exactly twelve fixed tones, in each septave, a most curious circumstance which is *universal* and *unalterable*. In order clearly to explain what I allude to, let us suppose a keyed instrument, the lowest key of which is C, to have exactly eight C s. Let us also suppose that there be placed, in the same room, a second, and also a third instrument; each of which is similar to the former in every respect, except as to the method of tuning.

Let us suppose that the first instrument be tuned in the following manner. Let us begin by tuning the lowest C to any given pitch. Then let us make all the successive octaves, C C, C C, &c. quite perfect. We shall then have seven successive octaves, which will bring us to the upper C of this instrument.

Now, let us suppose that the second instrument be tuned as follows. Let us begin, as in the former case, by tuning the lowest C; and let us give it precisely the same pitch as the lowest C of the first instrument. Then let us make the

† Thirds are divided into major thirds and minor thirds. A major third is composed of four intervals, (or four half-tones, as they are commonly called,) and a minor third is composed of three.

There are two species of major thirds; namely, perfect thirds, and imperfect thirds. And the imperfect thirds are also of two species; viz. sharp thirds, and flat thirds.

Example. If, in a tuning instrument which has two wires of equal thickness, the length of some C wire be 300, and if the length of the corresponding E wire, under the same tension, be 240; then, C E will be a *perfect third*. If the E wire be shorter, (such as 239,) then C E will be a *sharp third*; that is to say, a third *sharper* than the perfect third. But, if the E wire be longer, (such as 241,) then C E will be a *flat third*; that is to say, a third *flatter* than the perfect third.

The minor thirds are likewise of two species; namely, perfect minor thirds, and imperfect minor thirds. And the imperfect minor thirds are (in like manner as the imperfect major thirds) of two species also; viz. sharp minor thirds, and flat minor thirds.

interval between that C and the G next above a perfect quint. And then, in like manner, let us tune all the following intervals by perfect quints, until we get to C again. We shall then have twelve perfect quints, in the following order, which will bring us to the upper C of this second instrument, viz.

C, G; — G, D; — D, A; — A, E; — E, B; — B, F sharp, which is the same key as G flat†; — G flat, D flat; — D flat, A flat; — A flat, E flat; — E flat, B flat; — B flat, F; — F, C.

Now, let us suppose that the pitch of the upper C in the first instrument be carefully compared with the pitch of the upper C in the second instrument, and it will be found that those two Cs are not *in unison.* For, the upper C in the second instrument, which was pitched by means of the twelve perfect quints, will be found to yield a *more acute* sound than the upper C in the first instrument, which was pitched by means of the seven perfect octaves. Or, in other words, the C twelfth quint in the second instrument will be *sharper* than the C seventh octave in the first instrument.

The difference of pitch between those two high Cs will be rendered far more perceptible to the ear, by transferring that difference to the lower octaves. Therefore, let us suppose that the third instrument be tuned in the following manner, viz. Let us begin by tuning the upper C in this third instrument to the same pitch exactly as the C twelfth quint, or upper C, in the second instrument. And then, beginning from the upper C in this third instrument, let us tune all the other Cs in it successive perfect octaves descending.

Now, if we strike one of the Cs in this third instrument, about the middle octave, or lower down, and if at the same time we strike the corresponding C in the first instrument, then the C in the third instrument, which was derived from the C twelfth quint in the second instrument, will yield a *more acute* sound than the corresponding C in the first instrument which was derived from the octaves. A beating will be heard between them; and a kind of disagreeable sound will be produced, not very unlike the howling of a WOLF at a distance. Now, *the difference of pitch, between the C derived from the quints and the corresponding C derived from the octaves, is what is technically called, by tuners, THE WOLF.*

Musicians and tuners are in the habit of talking of THE WOLF in the singular number. I shall, however, shew in the sequel that there are as many as FIVE WOLVES, in the quints and major thirds taken together, in all those instruments which have exactly twelve fixed keys, or exactly twelve fixed tones, in each septave. Now, in order to distinguish these FIVE from each other, *the difference of pitch, between the C derived from the quints and the corresponding C derived from the octaves,* (which is, by tuners, commonly termed THE WOLF,) *is what I shall call THE QUINT WOLF.* See the following TABLE.

† Whenever I say that F sharp is the *same key* as G flat, or that G sharp is the *same key* as A flat, &c. I always mean that it is so in a keyed instrument which has exactly twelve fixed keys in each septave.

TABLE OF OCTAVES AND OF QUINTS.

The length of a wire which would yield the same sound as the lowest **C**, (or fourth bass **C**,) in the first and second instruments, is represented, in this TABLE, by the number 960.

	Quarters of an inch.	Hundredths of one quarter of an inch, and decimal parts of one hundredth of one quarter of an inch.
ASCENDING PERFECT OCTAVES IN THE FIRST INSTRUMENT. *(Read them from the bottom upwards.)*		
These numbers are found by taking *halves* successively. See page 3.		
7th octave, *or* 3d treble C.	7	50,
6th octave, *or* 2d treble C.	15	
5th octave, *or* 1st treble C.	30	
4th octave, *or* middle C.	60	
3d octave, *or* 1st bass C.	120	
2d octave, *or* 2d bass C.	240	
1st octave, *or* 3d bass C.	480	
Lowest C, *or* 4th bass C.	960	
ASCENDING PERFECT QUINTS IN THE SECOND INSTRUMENT. *(Read them from the bottom upwards.)*		
These numbers are found by taking *two thirds* successively. See page 4.		
12th quint, *or* 3d treble C.	7	39, 905.276.408.858.179.929.662.935 +
11th quint, *or* 2d treble F.	11	09, 857.914.613.287.269.894.494.402 +
10th quint, *or* 1st treble B flat.	16	64, 786.871.919.930.904.841.741.604 +
9th quint, *or* 1st treble E flat.	24	97, 180.307.879.896.357.262.612.406 +
8th quint, *or* middle A flat.	37	45, 770.461.819.844.535.893.918.609 +
7th quint, *or* middle D flat.	56	18, 655.692.729.766.803.840.877.914 +
6th quint, *or* 1st bass G flat.	84	27, 983.539.094.650.205.761.316.872 +
5th quint, *or* 2d bass B.	126	41, 975.308.641.975.308.641. *ad infin.*
4th quint, *or* 2d bass E.	189	62, 962.962. *ad infinitum.*
3d quint, *or* 3d bass A.	284	44, 444.444. *ad infinitum.*
2d quint, *or* 3d bass D.	426	66, 666.666. *ad infinitum.*
1st quint, *or* 4th bass G.	640	
Lowest C, *or* 4th bass C.	960	
DESCENDING PERFECT OCTAVES IN THE THIRD INSTRUMENT, derived from the twelfth quint in the second instrument. *(Read them downwards.)*		
Upper C, *or* 3d treble C.	7	39, 905.276.408.858.179.929.662.935 +
These numbers are found by successive *doubling*. See page 3.		
1st octave, *or* 2d treble C.	14	79, 810.552.817.716.359.859.325.870 +
2d octave, *or* 1st treble C.	29	59, 621.105.635.432.719.718.651.741 +
3d octave, *or* middle C.	59	19, 242.211.270.865.439.437.303.482 +
4th octave, *or* 1st bass C.	118	38, 484.422.541.730.878.874.606.964 +
5th octave, *or* 2d bass C.	236	76, 968.845.083.461.757.749.213.929 +
6th octave, *or* 3d bass C.	473	53, 937.690.166.923.515.498.427.859 +
7th octave, *or* 4th bass C.	947	07, 875.380.333.847.030.996.855.718 +
The *difference of pitch*, between the C derived from the quints and the corresponding C derived from the octaves, is THE QUINT WOLF.		
The length of the wire, for example, which would yield the sound of the first bass C derived from the octaves, is	120	
The length of the wire, which would yield the sound of the corresponding first bass C derived from the quints, is	118	38, 484.422.541.730.878.874.606.964 +
Consequently, *the difference between those two lengths* shews the value of the corresponding QUINT WOLF.	1	61, 515.577.458.269.121.125.393.035 +

There are FOUR OTHER WOLVES in the major thirds. But, in order to explain this part of the subject, it will be necessary first to shew that there are four series of major thirds which are *unalterably distinct* from each other. Those four series are shewn, in four columns, in the following TABLE OF SUCCESSIVE MAJOR THIRDS, where the four keys C, G, D, A, are placed, in the lowest line, in the order of occurrence as successive quints. The other keys are then to be placed, as major thirds, in the regular order shewn in the TABLE.

TABLE OF SUCCESSIVE MAJOR THIRDS.

(Read each column from the bottom upwards.)

FIRST, or C COLUMN.	SECOND, or G COLUMN.	THIRD, or D COLUMN.	FOURTH, or A COLUMN.	HIGHER OCTAVES of the keys in the first column.
Middle C.	Middle G.	First treble D.	First treble A.	Second treble E.
First bass A flat.	Middle E flat.	Middle B flat.	First treble F.	Second treble C.
First bass E.	First bass B.	Middle G flat.	First treble D flat.	First treble A flat.
First bass C; or key-note.	First bass G.	Middle D.	Middle A.	First treble E.

By inspecting the above simple TABLE, it will clearly appear that the following series of major thirds, viz. C, E; — E, G sharp, which is the same key as A flat; — and A flat, C, forms a column, in which those three successive major thirds return in constant and regular succession, without ever including in that series any of the other nine major thirds. So that, nature has, as it were, (if I may be allowed the expression,) *imprisoned* that series of three major thirds in a column by itself.

The series of the three major thirds, G, B; — B, D sharp, which is the same key as E flat; — and E flat, G, forms a second column.

The series of the three major thirds, D, F sharp, which is the same key as G flat; — G flat, B flat; — and B flat, D, forms a third column.

And, in like manner, the series of the remaining three major thirds, A, C sharp, which is the same key as D flat; — D flat, F; — and F, A, forms a fourth column.

In order to distinguish these four columns from each other, I shall name them from the lowest key in each column respectively; thus, we shall have,

1st, The C column, which consists of C, E, and A flat.
2dly, The G column, which consists of G, B, and E flat.
3dly, The D column, which consists of D, G flat, and B flat.
4thly, The A column, which consists of A, D flat, and F.

Now, it is a very curious fact, that *each* of these four columns of major thirds has ITS OWN DISTINCT WOLF, exclusively of the QUINT WOLF, which, as we have already seen, affects all the twelve keys without exception.

If, in the C column, for example, the three successive major thirds, C, E; — E, G sharp, which is the same key as A flat; — and A flat, C, be all made quite perfect; then, CC, which is thus produced by means of those three perfect thirds, will not be a perfect octave, but it will be *flatter* than the perfect octave CC would be. *The difference of pitch, between the C derived from the major thirds and the C octave corresponding to it, is what I shall call THE C WOLF*†. I shall denominate it thus, because it belongs to the C column. There will be found an exactly similar WOLF in each of the other three columns. These FOUR WOLVES may very properly be distinguished by the names of the four columns to which they respectively belong; thus, we shall have,

1st, In the C column, THE C WOLF.
2dly, In the G column, THE G WOLF.
3dly, In the D column, THE D WOLF.
4thly, In the A column, THE A WOLF.

	Quarters of an inch.	Hundredths of one quarter of an inch.
† If, for example, the length of the wire which would yield the sound of the first bass C be - - - - - - - - - -	120	
Then, the lengths of the same wire, under the same tension, which would respectively yield the sounds of the three successive perfect major thirds, would be as follows, viz.		
First bass E. - - - - - - - - - - - -	96	
First bass G sharp, which is the same key as the first bass A flat.	76	80
The middle C which is thus derived from the major thirds.	61	44
(Those numbers are found by taking *four fifths* successively.)		
Now, the length of that wire which would yield the sound of the *true* middle C; namely, the sound of the perfect octave above the first bass C, would be only - - - - - - - - -	60	
Consequently, *the difference between those two last mentioned lengths* shews the value of the corresponding C WOLF. - -	1	44

And those FOUR, together with THE QUINT WOLF, make the FIVE WOLVES which I have mentioned above. And it was from my having observed these FIVE DISTINCT WOLVES that I was led to find out that superior mode of tuning keyed instruments which I am now going to describe.

If, in any musical instrument which has exactly twelve fixed keys or twelve fixed tones in each septave, the octaves be tuned perfect; then, the quints cannot all be tuned perfect, as the TABLE OF OCTAVES AND OF QUINTS, in page 6, clearly shews. And if, in any such instrument, the quints be tuned perfect; then, the octaves cannot be tuned perfect.

Now, such is the construction of the human ear, that we can bear to hear a *much greater* deviation from perfection in the quints than we can bear to hear in the octaves. And we can bear a *still greater* deviation from perfection in the major thirds than we can bear either in the octaves or in the quints. Musicians and tuners have therefore agreed not to tune all the quints perfect, as that would make the octaves intolerable.

Some tuners, however, in order to assist the quints, have very improperly proposed to tune the octaves a little *imperfect*. The objections to this method are obvious. For, if we sharpen the octaves to assist the quints, it injures the thirds; and if we flatten the octaves to assist the thirds, it injures the quints.

Besides, however small the deviation from perfection may be in a single octave, it will become very sensible in two or three; and, in the extent of six or seven octaves, such a deviation will become very offensive.

It is necessary, therefore, that all the octaves should be tuned *perfect*. Now, from what has been already said, it is evident that this object can be obtained in one way only; and that is, by tuning some one, or more, of the twelve quints above mentioned *flatter than perfect*.

In order for all the octaves to be tuned *perfect*, it is likewise necessary that some one, or more, of the three successive major thirds, in each of the four natural columns above mentioned, must be tuned *sharper than perfect*. As for example, in the C column, some one, or more, of the three successive major thirds, viz. C, E; — E, G sharp, which is the same key as A flat; — and A flat, C, must be tuned *sharper than perfect;* for, otherwise, CC, which is produced by means of those three successive major thirds, could not be (as it ought to be) a perfect octave; because three successive perfect thirds do not make up a perfect octave. Therefore, in tuning any musical instrument which has exactly twelve fixed keys or twelve fixed tones in each septave, the problem does, of necessity, resolve itself into this; namely, to ascertain which one, or more, of the three successive major thirds, in each of the above mentioned four natural columns respectively, ought to be tuned *sharper than perfect,* and in what proportion each is to be so tuned; and also, which one, or more, of the twelve successive quints ought to be tuned *flatter than perfect,* and in what proportion.

What I have just stated will enable the reader easily to understand what musical TEMPERAMENT is. This term signifies *the precise adjustment of*

the relative pitch of all the keys or fixed tones of any musical instrument, so as to distribute the four wolves which are in the major thirds, and likewise the quint wolf, according to some given systematic proportions. And the OBJECT OF TEMPERAMENT is to adjust the relative pitch of all the keys or fixed tones in such a due proportion as to render the instrument harmonious and melodious in the highest degree possible. This is an object of very great consequence to the musical world. For, the finest keyed instrument, even if it be played upon in the most capital manner, loses, either when *out of tune,* or when tuned according to an *improper temperament,* the power of producing those delightful and exquisite sensations which the very same instrument becomes capable of producing when it is tuned scientifically and correctly.

There are a great number of different modes of TEMPERAMENT, which may be classed as follows, viz. the EQUAL TEMPERAMENT, and the UNEQUAL TEMPERAMENTS.

That mode of temperament in which the quint wolf is distributed in an equal proportion amongst all the twelve quints of an instrument is that which is called THE EQUAL TEMPERAMENT. And every other mode of temperament, without exception, is called an UNEQUAL TEMPERAMENT.

In that mode of tuning which is called THE EQUAL TEMPERAMENT, all the twelve quints are made imperfect; for, every one of them is tuned *flatter than perfect.* But, in my mode of tuning, for example, which is one of the UNEQUAL TEMPERAMENTS, there are, as will be explained in the sequel, seven quints quite *perfect,* and five quints *flatter than perfect.*

The consequence of an equal distribution of the QUINT WOLF, in THE EQUAL TEMPERAMENT, is, that the C WOLF will become, of necessity, equally distributed amongst the three major thirds in the C column; and that the G, D, and A WOLVES will become equally distributed amongst the three major thirds in the G, D, and A columns respectively.

THE EQUAL TEMPERAMENT is, however, a mode of tuning which I very much disapprove. According to that erroneous system, there is not a single *perfect* third, nor a single *perfect* fourth, nor a single *perfect* quint, in the whole instrument. That charming and delightful harmony and melody which a proper mode of tuning enables fine players to produce, is thus rendered, in every case, impossible. All those chords, which nature has rendered perfect, are, by this objectionable contrivance, rendered imperfect. And in those instruments where chords are made to sound for a considerable time, such for instance as organs, the imperfection of THE EQUAL TEMPERAMENT is most striking. Perfect chords are pleasing to the ear, they strike to the heart, and they are founded in the very nature of musical sounds. But, by that mode of tuning which is called THE EQUAL TEMPERAMENT, all those regular coincidencies of those duly proportioned vibrations which produce true concords are destroyed, and every thing is discord. Let us take a quint, as an example explanatory of this fact. We have already seen, that, when the wire in a well constructed monochord is

reduced to two thirds of its length, the sound produced by the two thirds will be that of a perfect quint. Now, the number of vibrations of a wire of uniform thickness, which has any given degree of tension, will be *inversely* as the length of the wire. That is to say, that a wire, for example, of thirty inches long, will vibrate twice, during the time that an equal wire, of twenty inches long, under the same tension, will vibrate three times. If the longer wire be tuned to C, the shorter wire will be the G perfect quint. It follows from this, that the *third, sixth,* and *ninth* vibrations, &c. of the G wire so tuned, will respectively keep pace with the *second, fourth,* and *sixth* vibrations, &c. of the C wire above mentioned. And it is from that very circumstance that concords are produced. But, let us suppose, that the length of the G wire, instead of being precisely two thirds of the length of the C wire, be either something more, or something less; then, the *third, sixth,* and *ninth* vibrations, &c. of the G wire, will no longer respectively keep pace with the *second, fourth,* and *sixth* vibrations, &c. of the C wire. Therefore, instead of concords, discords will be heard. For, true concords can only be obtained by means of exact geometrical proportions between the vibrations produced. —— There are various classes of discords. Some of them are offensive; others are not. Some of them are characteristic; others are not so. How to limit the extent of the discordancies, and how to distribute the non-offensive discords in such a manner that the various keys, instead of being injured, shall on the contrary be improved, will be very fully explained hereafter. But, to have, in any instrument, nothing but discords, is abominable; and that is always and necessarily the case, whenever that mode of tuning which is denominated THE EQUAL TEMPERAMENT is adopted.

By the scheme of EQUAL TEMPERAMENT, not only every third is sharp, but is equally sharp; every fourth is sharp, and is also equally sharp; and every quint is flat, and is equally flat. Therefore, not only every major key is rendered imperfect, but is rendered similarly and equally imperfect. This of course destroys *the difference of character* which ought to exist, in a well tuned instrument, between the different major keys. And the minor keys are liable to the same defect, for a similar reason. Thus it is that dull monotony is substituted for pleasing and orderly variety. And modulation from key to key loses, in great measure, the very object of modulation, which is to relieve the ear, and to cause us to return to the original key with an increased pleasure, which arises from the systematic variety of the different keys through which we have successively passed.

Some tuners, who prefer an UNEQUAL TEMPERAMENT, throw the whole of the QUINT WOLF into the key of E flat. Others, divide it between the two keys of A flat, and D flat; or between some other two keys. Those persons generally say, that they throw THE WOLF (as they inaccurately call it) into a single key, or into two keys, according to their respective method of tuning. The absurdity of this assertion must now be apparent to the reader. For, since there is not only in the quints a QUINT WOLF, but there are likewise in the major thirds FOUR DISTINCT WOLVES, each of which four is by nature confined

to *its own peculiar and unalterable column;* it is obvious that the D WOLF, for instance, cannot ever be taken from its own column, to be carried, either to the key of E flat, in the G column; or to the key of A flat, in the C column; or to the key of D flat, in the A column. Such temperaments are, therefore, unsatisfactory in theory. They are also defective in practice; for, they render some keys too bad to modulate into them without offending the ear extremely.

In my new method, there are none of those defects. Every key in my TEMPERAMENT is made pleasing, and fit for transposition and modulation, and has also a *peculiar character* which belongs to it. Some keys are fitted for chearful music; some, for grave. Some, for martial; some, for pastoral. Some, for soft or melodious pieces; and some, for those which are solemn, plaintive, or majestic. How is it possible that the same key, or twelve keys of exactly the same character, should be capable of giving to compositions of directly opposite characters, that effect which is the best suited to each composition respectively?

In order to introduce the greatest degree of variety which is consistent with proper harmony and with proper melody, it is evident that some one key, at least, should be made as *perfect* as possible. If so, the key of C, which so frequently recurs, ought to be that key. It is besides the only key which has neither flats nor sharps in the common construction of keyed instruments.

I, therefore, make the key of C, with a perfect third, C, E; with a perfect fourth, C, F; and with a perfect quint, C, G. —— In tuning, I proceed thus.

First. I begin by pitching the first bass C, to my tuning-fork, tuning-glass, or monochord. I consider the first bass C as the key-note. I then make the next C above, which is called the middle C, a perfect octave from the first bass C.

Or, I pitch the middle C, to my tuning-fork, tuning-glass, or monochord; and I then pitch the first bass C, as a perfect octave next below. It is of no consequence which of those two equivalent methods be used to obtain the pitch of those two Cs. But, in order to tune the other keys of the instrument, I prefer starting from the first bass C, as the key-note; instead of starting from the middle C; because the beatings are more perceptible to the ear, in the former case, than they would be in the latter. —— The reader will do well to turn to the TUNING TABLE, in page 15, and to follow it in reading the pages 12, 13, and 14.

Secondly. From the first bass C, I make C, G, a perfect quint.

Thirdly. From the first bass C, I make C, E, a perfect third. And I then tune the two octave Es next above.

Fourthly. From E, I make E, B, a perfect quint; and I prove B, from G, as a perfect third.

Fifthly. From the middle C, I tune C, F, upwards, a perfect fourth; or (what is equivalent) I tune F, C, downwards, a perfect quint. I then tune the F next above, a perfect octave.

Sixthly. The pitch of F being determined, I tune F, B flat, upwards, a perfect fourth; or (what is equivalent) B flat, F, downwards, a perfect quint.

Seventhly. I then pitch A flat, exactly half way between E and the C next above. If a monochord be used for this purpose, the length of the wire A flat must be made *a geometrical mean proportional* between the length of the wire E and the length of the wire C next above. The two sharp thirds produced by this method are peculiarly suited to solemn and to plaintive music. The effect was remarkably striking, in a comparative experiment which I shall relate in the sequel. I was so struck with the peculiar excellency of sharp thirds of this exact value for musical compositions which are either plaintive or majestic, that I have, for the sake of accuracy and methodical discrimination, given them a distinct name. *If, from any perfect octave, we deduct one perfect third; and if we then divide the remaining interval into two thirds equally sharp; each of those two thirds, as well as every other third of that same degree of sharpness, is that sharp third which I shall call a* BI-EQUAL THIRD.

If the pitch of A flat be not determined by means of a monochord, but simply by the ear, its pitch may be ascertained with great precision, if the tuner pay exact attention to the equality of the beatings of the two successive major thirds, E, G sharp, which is the same key as A flat; and A flat, C.——In tuning each key throughout the whole instrument, too much attention cannot be paid to the beatings, as that is by far the most accurate way of tuning by the ear. For, whenever either a third, a fourth, a quint, or an octave is quite *perfect*, there is, in such case, no beating to be heard. But, on the contrary, whenever either of them is in any degree *imperfect*, but is not too distant from perfection, a beating is always audible.——A very slow beating proves that the deviation from perfection is not great. A quicker beating shews that the deviation from perfection is more considerable. And, from the equality of the beatings, equal deviations from perfection may be correctly ascertained.

Eighthly. The pitch of A flat being now determined, I next pitch A flat, E flat, upwards, a perfect quint; or I tune E flat, A flat, downwards, a perfect fourth. E flat will then be exactly half way between B and the G next above.

Ninthly. The pitch of A flat being determined, as explained above; I tune A flat, D flat, upwards, a perfect fourth; or I tune D flat, A flat, downwards, a perfect quint. I then tune the D flat next above, a perfect octave.

Tenthly. The pitch of D flat being determined, I tune D flat, G flat, upwards, a perfect fourth; or I tune G flat, D flat, downwards, a perfect quint.

Thus, I have already got seven quints quite *perfect;* viz. 1. C, G; 2. E, B; 3. F, C; 4. B flat, F; 5. A flat, E flat; 6. D flat, A flat; 7. G flat, D flat.

I have likewise got two quints very nearly perfect, but a little flat, viz. 1. B, F sharp, which is the same key as G flat; 2. E flat, B flat.

Each of those two quints differs from a perfect quint, only ONE in *two thousand six hundred and fifty seven parts and a half* nearly; or only about 1.128.831 parts in 3.000.000.000. See the value of those two quints, in page 23.

It is a fact very worthy of notice, that, in each of those two last mentioned quints, *two distinct beatings* are to be heard at the same time. The one is very

slow, and the other is considerably quicker. Now, as each of those two quints does, as proved by the monochord, approach so very nearly to perfection; it is evident, that it is the *slower* beating which is the proper beating to be attended to in the case of each of those two quints which are so very nearly perfect.

I have a scientific way of determining the three remaining quints that are also flat, which I shall now explain. They are, 1. G, D; 2. D, A; 3. A, E.

We have already seen that the pitch of G has been determined, as a perfect quint from the first bass C. The pitch of E has likewise been determined, as a perfect third from that same C. The E first octave above from that E, and the E second octave above from that same E, are of course determined likewise.

Eleventhly and twelfthly. It is now requisite so to pitch the D, and the A, between the G perfect quint from C, and the E second octave from that E which is the perfect third from C, in such a manner, that the interval G, E, may be divided into three equally flat quints, G, D; D, A; and A, E. None of those three quints are of such a degree of flatness as to be offensive to the ear; for, each of those three quints differs from a perfect quint, only ONE in *three hundred and sixty one parts and a half* nearly; or only about 8.298.850 parts in 3.000.000.000. See the value of those three quints, in page 23. —— If a monochord be used to determine the pitch of D, and of A; then, the length of the wire D, and the length of the wire A, must be made *two geometrical mean proportionals,* between the length of the wire G, and the length of the wire E. But, if a monochord be not used for this purpose, and if the tuner determine the pitch of D, and of A, by the ear; it may be done with great accuracy, if he attend properly to the equality of the beatings of the three successive flat quints, G, D; D, A; and A, E. That fact has been ascertained by repeated trials.

If the interval G, E, be (as in Kirnberger's method of tuning) divided into one perfect quint, and two equally flat quints; such, for instance, as the perfect quint G, D, and two equally flat quints, D, A, and A, E; then, each of those two flat quints, by becoming too flat, is offensive to the ear. I have made that experiment with care; and the result was what I have just mentioned. And if the same interval G, E, be divided into two perfect quints, and one flat quint; then, the flat quint, so produced, is still more offensive. This was fully to be expected.

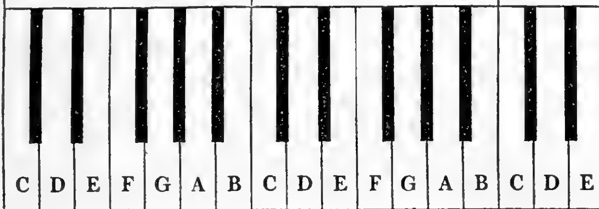
I shall now give a definition of those three equal and non-offensive quints which result from the scientific division of the interval G, E, above explained, in order clearly to distinguish it from the two exceptionable divisions just described. That is to say; *if the interval between the perfect quint from a key-note, and the second perfect octave above the perfect third from the same key-note, be divided into three equally flat quints; each of those three equal quints is that which I shall call a TRI-EQUAL QUINT. And the inverse fourth of any tri-equal quint is that which I shall call a TRI-EQUAL FOURTH.*

My NEW MODE OF TUNING, which I have just minutely explained, is exhibited in a very convenient manner in the following TABLE.

TUNING TABLE;

Shewing, at one view, the MANNER and ORDER of tuning the twelve keys of an instrument, according to the STANHOPE TEMPERAMENT.

As soon as a key is tuned, it is represented by a CAPITAL LETTER.
The small letters *represent those keys which are going to be tuned.*

Order of tuning.	First bass septave.	The middle septave.	First treble septave.
			
First	C ———	c	
Secondly	C —— g		
Thirdly	C — e ———	——— e ———	——— e
Fourthly	...... E —— b		
Fifthly	.... f ———	C —— f	
Sixthly	.... b flat	——— F	
Seventhly	...... E —— a flat —	C	
Eighthly	..... e flat —— A flat ——	—— e flat	
Ninthly	.. d flat ——— A flat —	— d flat	
Tenthly	.... g flat ——	— D flat	
Eleventhly } *Twelfthly*	.... G ——	—— d ——— a ——	—— E

First .... C, c, perfect octave.
Secondly .... C, g, perfect quint.
Thirdly .... C, e, perfect third. And e, e; — e, e, two perfect octaves.
Fourthly .... E, b, perfect quint.
Fifthly .... f, C, perfect quint. And f, f, perfect octave.
Sixthly .... b flat, F, perfect quint.
Seventhly .... E, a flat; — a flat, C, two bi-equal thirds. See page 13.
Eighthly .... A flat, e flat, perfect quint.
Ninthly .... d flat, A flat, perfect quint. And d flat, d flat, perfect octave.
Tenthly .... g flat, D flat, perfect quint.
Eleventhly and twelfthly ... G, d; — d, a; — a, E, three tri-equal quints. See page 14.

After I have tuned, in the manner above particularly specified, those septaves which are about the middle of the instrument; I then tune all the octaves perfect, both in the upper and in the lower septaves. If it be a piano-forte, or any other instrument which has two or more strings to each key; then, great attention should be paid to the tuning of those unisons perfect.

When the instrument is tuned, according to the rules above mentioned; the octaves should be tried as follows. Strike four Cs at once all through the instrument. Then four Ds, &c. successively. Try the short keys all through the instrument, in like manner. And if the instrument will not stand this test, we may be quite certain that it is *not in tune.*

It will not be necessary for me to go regularly through the twelve fourths; as they always follow their corresponding quints. Thus, a *perfect quint*, such as the perfect quint, for example, from B flat, namely, B flat, F, will give, for the inverse key F, a *perfect fourth*, viz. F, B flat next above. And a *flat quint*, such as the flat quint from D, viz. D, A, will give, for the inverse key A, a *sharp fourth*, namely, A, D next above. And the degree of sharpness of the sharp fourth will always, of necessity, correspond with the degree of flatness of the flat quint, wherever a flat quint occurs. My mode of tuning does not produce a single offensive quint; neither does it produce a single offensive fourth.

Having accurately explained how the NEW TEMPERAMENT which I have discovered regulates the twelve quints, and also the twelve fourths; I will now shew in what manner it affects the twelve thirds. But, before I can make myself clearly understood upon this interesting and curious part of the subject, I must first state a few things, by way of preliminary observations.

I have established the following facts, from experiments made with the greatest attention, by means of a very excellent monochord, and also by means of a grand piano-forte, built by one of the best piano-forte makers in England.

I divided the perfect octave CC, between the first bass C and the middle C, into three successive thirds, in the three following ways, viz.

First. I divided that perfect octave CC, into two perfect thirds, namely, C, E; and E, G sharp, which is the same key as A flat; and into one third sharper than a perfect third, namely, A flat, C next above. This sharper third, so produced, was, as I expected, very offensive.

Secondly. I divided that same perfect octave CC, into one perfect third, C, E; and into two equally sharp thirds, viz. E, G sharp, which is the same key as A flat; and A flat, C next above. The length of the wire A flat, in the monochord, was made *a geometrical mean proportional* between the length of the wire E, and the length of the wire C next above. This last mentioned division of the octave CC is excellent; for, the two sharp thirds thus produced were *bi-equal thirds,* as was particularly explained above. See page 13.

Thirdly. I then divided the same perfect octave CC, into three thirds, all equally sharper than perfect. This equal division was effected, by the wires E, and A flat, in the monochord, having been made *two geometrical mean*

proportionals between the length of the wire C, and the length of the wire C next above. Those three equally sharp thirds, of this last mentioned value, are the very same as the sharp thirds which result from that mode of tuning which is commonly termed THE EQUAL TEMPERAMENT. Not one of those three equally sharp thirds has either of those two striking characters which are to be found in an instrument that is tuned in the most advantageous manner. For, not one of those three equally sharp thirds has either the beauty of a third which is *perfect*, nor the peculiar and solemn character of that other third which I have already denominated a *bi-equal third*. This is, therefore, another inherent defect belonging to that method which is called THE EQUAL TEMPERAMENT; inasmuch as that defect necessarily extends to all the twelve keys, in that ill contrived mode of tuning.

I can now explain to the reader in what manner the NEW TEMPERAMENT which I have discovered affects the twelve thirds, in a keyed instrument which has exactly twelve † keys in each septave.

Two of those twelve thirds are quite perfect. Those two are, 1. C, E; 2. G, B.

Six of the remaining ten thirds are sharper than perfect; and each of them is of the exact value of a *bi-equal third*. Those six are, 1. E, G sharp, which is the same key as A flat; 2. A flat, C; 3. B, D sharp, which is the same key as E flat; 4. E flat, G; 5. G flat, B flat; 6. D flat, F.

The remaining four thirds are likewise sharp, but less so than the preceding six; and each of them is, in respect to sharpness, *intermediate* between the other two classes above specified; namely, *intermediate* between a perfect third and a bi-equal third. See page 23.

Of those four last mentioned sharp thirds, two are nearer to a bi-equal third than to a perfect third. Those two are, 1. A, C sharp, which is the same key as D flat; 2. B flat, D. See page 23.

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† The usual number of *twelve keys* seems to be pointed out to us from the natural number of *twelve musical intervals*. —— There are three reasons for considering that division of the septave as a *natural* one.

First. If we start from any key, (such, for example, as from C,) then, *twelve* quints, duly tempered according to my natural and scientific TEMPERAMENT explained above, will bring us again to a key of the same denomination. Therefore, the number of *twelve* successive quints naturally leads to the number of *twelve keys* of *twelve* distinct denominations.

Secondly. There are four natural sets or columns of major thirds, and each of those four sets respectively contains three series of major thirds, as was fully explained above. See pages 7 and 8. This makes up the same number of *twelve keys*.

Thirdly. The like number of *twelve keys* is found also in the following manner; namely, by considering the three natural sets or columns of minor thirds; for, each of those three sets respectively contains four series of minor thirds, making likewise *twelve keys* in all, viz.

First set. 1. C, E flat; 2. E flat, G flat; 3. G flat, A; 4. A, C.
Second set. 1. D flat, E; 2. E, G; 3. G, B flat; 4. B flat, D flat.
Third set. 1. D, F; 2. F, A flat; 3. A flat, B; 4. B, D.

And, of the four *intermediate* sharp thirds above mentioned, the remaining two are nearer to a perfect third than to a bi-equal. Those two last mentioned thirds are, 1. D, F sharp, which is the same key as G flat; 2. F, A. See page 23.

The reader will therefore perceive, that, in my TEMPERAMENT, the most easy keys have, for thirds of the key, those thirds which are the most perfect.

The two thirds of the key which are quite perfect are in the following keys, viz.

C major, which is without either flat or sharp;
G major, which has only one sharp.

The two sharper thirds of the key which are the nearest being perfect are in the following keys, viz.

D major, which has only two sharps;
F major, which has only one flat.

The two still sharper thirds of the key which come next in respect to sharpness are in the following keys, viz.

A major, which has three sharps;
B flat major, which has two flats.

And those six sharpest thirds of the key, each of which I term a *bi-equal third*, are in the following keys, viz.

E major, which has four sharps;
B major, which has five sharps;
E flat major, which has three flats;
A flat major, which has four flats;
D flat major, which has five flats;
G flat major †, which has six flats.

As there is not, in my TEMPERAMENT, any third, any fourth, or any quint, less perfect than those specified above and in the following TABLES; it is evident that this new mode of tuning must be excellent for transposition, and for modulation. This fact has been established by regular and repeated experiments, made in presence of many of the best judges. Between sixty and seventy of the very first professional persons, of both sexes, and of the ablest connoisseurs in England, have given to this NEW TEMPERAMENT their decided approbation. It answers well, both in the major and in the minor keys.

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† Some authors call the key of G flat major, the key of F sharp major with six sharps. But it is more systematic and methodical, to consider (as Dussek does) the five short keys as flat keys in the major mode.

There are some few facts, in particular, which throw such light upon the science of music, that I think it proper to mention them. On an excellent piano-forte, tuned in my manner, that favourite portuguese hymn, called *Adeste Fideles*, which is commonly printed in A major, was played successively in that key, in the key of A flat major, in C major, and in D flat major. The following was the result of this comparison.

First, the piece was the most characteristic and sublime in the key of A flat. It was better in that key even than in the original key of A.

Secondly, the hymn was comparatively intolerable in the key of C; although, according to my TEMPERAMENT, the key of C is tuned *perfect;* having a perfect third, a perfect fourth, and likewise a perfect quint.

Thirdly, the piece was better even in the key of D flat than in the key of C; although the pitch of the key of D flat (being higher up) is less suited to the character of that solemn composition than the pitch of the key of C.

That is to say, in other words, that the intermediate key of C, although it be tuned *perfect*, is less proper for that piece of music than either D flat or A flat; one of which has a higher, and the other a lower pitch. And this fact is the more remarkable, on account of the following illustrative circumstance. Each of those three major keys, namely, D flat, C, and A flat, has, according to my TEMPERAMENT, a perfect fourth, and a perfect quint. Therefore, it is evident that the difference in the effect produced could result neither from the respective fourths, nor from the respective quints.

Consequently, the striking difference between those three keys, which every person with a good ear must feel, results principally from the thirds, and from the sixths, in each key respectively. And those two keys, namely, D flat, and A flat, *where the third in each is imperfect, and is of the exact value of a bi-equal third*, are beyond comparison better suited to the solemn character of the hymn than the key of C, where the third is, on the contrary, tuned quite *perfect*.

We have been in the habit of considering what is commonly termed THE WOLF as an inherent imperfection in every instrument which has exactly twelve fixed keys in each septave. Whereas, the very remarkable fact just mentioned, and several others of a like kind, most clearly prove, that, so far from THE FIVE WOLVES being imperfections, it is precisely the proper distribution of those WOLVES which produces that charming and essential VARIETY OF CHARACTER, between the different keys, which is one of the chief requisites in a well tuned instrument.

Thus it is, that, from our ignorance and narrow prejudices, the *perfection* of the principles which are to be found in nature are by us very frequently unobserved. But the more thoroughly we learn to understand them, the more we ought to feel gratitude towards the SUPREME BEING for enabling us to perceive the *sublime excellence* of their wonderful arrangement.

TABLE OF ALL THE KEYS IN AN OCTAVE;

Shewing the systematic and characteristic VARIETY which is produced between them by means of the STANHOPE TEMPERAMENT.

KEYS.	THIRDS.	FOURTHS.	QUINTS.
C.	Perfect.	Perfect.	Perfect.
B.	Bi-equal third.	Perfect.	Very nearly perfect.
B flat.	Intermediate between perfect and bi-equal third, but nearer to bi-equal.	Very nearly perfect.	Perfect.
A.	Intermediate between perfect and bi-equal third, but nearer to bi-equal.	Tri-equal fourth.	Tri-equal quint.
A flat.	Bi-equal third.	Perfect.	Perfect.
G.	Perfect.	Perfect.	Tri-equal quint.
G flat.	Bi-equal third.	Very nearly perfect.	Perfect.
F.	Intermediate between perfect and bi-equal third, but nearer to perfect.	Perfect.	Perfect.
E.	Bi-equal third.	Tri-equal fourth.	Perfect.
E flat.	Bi-equal third.	Perfect.	Very nearly perfect.
D.	Intermediate between perfect and bi-equal third, but nearer to perfect.	Tri-equal fourth.	Tri-equal quint.
D flat.	Bi-equal third	Perfect.	Perfect.
C.	Perfect.	Perfect.	Perfect.

N. B. Of the six keys which have, for the sharp third, a bi-equal third, four have quints *quite perfect*, and two have quints *very nearly perfect*.

The three tri-equal quints have thirds of *different* values, and so have likewise the three tri-equal fourths.

MONOCHORD TABLE,

No. I.

Shewing the length of each wire, from the middle C, to the C next below, both inclusive; supposing that the wire which yields the sound of the last mentioned C be 120 quarters of an inch long between the two bridges of the monochord.

Keys.	Quarters of an inch.	Hundredths of one quarter of an inch, and decimal parts of one hundredth of one quarter of an inch.
Middle C.	60	
First bass B.	64	
B flat.	67	50,
A.	71	70, 247.592 +
A flat.	75	89, 466.384 +
G.	80	
G flat.	85	38, 149.682 +
F.	90	
E.	96	
E flat.	101	19, 288.512 +
D.	107	10, 927.200 +
D flat.	113	84, 199.576 +
C.	120	

MONOCHORD TABLE,

No. II.

Shewing the manner of setting off the whole monochord scale, *from a single point;* supposing that point to be the extremity of the length of the G wire.

N. B. *This mode of setting off the scale, on the steel rod, is more mechanically accurate than setting it off from more points than one.*

Keys.	Quarters of an inch.	Hundredths of one quarter of an inch, and decimal parts of one hundredth of one quarter of an inch.	
Middle C.	20		To be set off BACKWARDS.
First bass B.	16		
B flat.	12	50,	
A.	8	29, 752.407 +	
A flat.	4	10, 533.615 +	
G.	0		
G flat.	5	38, 149.682 +	To be set off FORWARDS.
F.	10		
E.	16		
E flat.	21	19, 288.512 +	
D.	27	10, 927.200 +	
D flat.	33	84, 199.576 +	
C.	40		

MUSICAL MEMORANDUM TABLE;

Shewing, at one view, the MANNER in which the THREE KEYS, in each of the FOUR NATURAL COLUMNS respectively, are affected by the STANHOPE TEMPERAMENT.

FIRST, or C COLUMN.	SECOND, or G COLUMN.	THIRD, or D COLUMN.	FOURTH, or A COLUMN.	DOUBLE OCTAVES above the C octave, the A flat, and the E, in the first column.
C octave.	G octave.	D octave.	A octave.	
T	R	R	R	
A flat 7 T	E flat 8 R	B flat 6 T	F 5 T	C
T	R	R	R	
E 3 T	B 4 R	G flat 10 T	D flat 9 T	A flat
T	R	R	R	
C 1 T Key-note.	G 2 T	D 11 T	A 12 T	E

The FIGURES *under* the letters shew the ORDER in which the keys ought to be tuned.

The thirteen Ts are those intervals which are actually TUNED.

The eleven Rs are those intervals which are not actually tuned, but which RESULT from those intervals which are actually tuned.

EXPLANATION OF THE MUSICAL MEMORANDUM TABLE;

With the value of the THIRDS *and* QUINTS *reduced to a common denominator.*

EXPLANATION OF THE MARKS *OVER* THE LETTERS.	Relative lengths of the wires.	Difference between those lengths.
Means a perfect third.	$\frac{2.400.000.000}{3.000.000.000}$	$\frac{600.000.000}{3.000.000.000}$
Means a sharp third which is intermediate between perfect and bi-equal, but nearer to perfect.	D, G flat, = $\frac{2.391.431.532\ +}{3.000.000.000}$	$\frac{608.568.467\ +}{3.000.000.000}$
	F, A, = $\frac{2.390.082.530\ +}{3.000.000.000}$	$\frac{609.917.469\ +}{3.000.000.000}$
Means a sharp third which is intermediate between perfect and bi-equal, but nearer to bi-equal.	A, D flat, = $\frac{2.381.549.471\ +}{3.000.000.000}$	$\frac{618.450.528\ +}{3.000.000.000}$
	B flat, D, = $\frac{2.380.206.044\ +}{3.000.000.000}$	$\frac{619.793.955\ +}{3.000.000.000}$
Means that sharp third which is called a bi-equal third.	$\frac{2.371.708.245\ +}{3.000.000.000}$	$\frac{628.291.754\ +}{3.000.000.000}$

EXPLANATION OF THE MARKS *AFTER* THE LETTERS.	Relative lengths of the wires.	Difference between those lengths.
—— Means a perfect quint.	$\frac{2.000.000.000}{3.000.000.000}$	$\frac{1.000.000.000}{3.000.000.000}$
• —— Means a flat quint which is very nearly perfect.	$\frac{2.001.128.831\ +}{3.000.000.000}$	$\frac{998.871.168\ +}{3.000.000.000}$
• • • • • Means that flat quint which is called a tri-equal quint.	$\frac{2.008.298.850\ +}{3.000.000.000}$	$\frac{991.701.149\ +}{3.000.000.000}$

N. B. The ten Ts to the *quints* shew that ALL the quints are TUNED; except the two quints, B, G flat; and E flat, B flat, marked thus • ——

The three Ts to the *thirds* shew that ALL the three successive thirds in the C column are TUNED.

And the two Rs to the *quints*, and the nine Rs to the *thirds*, shew that ALL the other quints and thirds RESULT from the *tuned* intervals.

PARTICULARITIES OF THE STANHOPE MONOCHORD.

First. The wire is not made either of brass or of iron, but of steel, which is very far superior. For, steel wire does not keep continually lengthening, as brass and iron wires do when they are stretched considerably.

Secondly. The wire in this monochord does not, as usual, pull downwards on the bridges, but the whole wire forms one straight and horizontal line, by which means the moveable bridge, which determines the exact length of the wire, can be moved without altering the tension of the wire. This is not the case when the wire pulls downwards on the bridges.

Thirdly. The ends of the wire are not twisted round the two stout steel pins which keep it stretched; but, each end of the wire is soft-soldered in a long groove formed in a piece of steel which goes over its corresponding pin. This is a great improvement.

Fourthly. One of those two steel pins is strongly fastened on a brass slider which is moved by means of a screw with very fine threads, which screw has a large micrometer head minutely divided on its edge, and a corresponding *nonius;* so that, the tension of the wire may be adjusted with the greatest precision, in order to obtain its exact pitch.

Fifthly. A slider is fixed across the top of the moveable bridge, and is moved by means of another screw with very fine threads; so that, the length of the wire may be regulated with the greatest nicety in all cases.

Sixthly. The above mentioned slider which is on the top of the moveable bridge is adjusted to the steel rod or scale, not by sight, or by the coincidence of lines; but by means of *mechanical contact* against projecting pieces of steel firmly fixed on that steel scale, which method is incomparably more correct. Those projecting pieces are fixed on that scale at the respective distances specified in the MONOCHORD TABLE, NO. I. See page 21.

Seventhly. Each bridge carries a metallic finger which keeps the wire close to the top of the bridge whilst the wire is made to vibrate.

Eighthly. The vibrations of the wire are produced by touching it with a piece of cork, with the same elastic force, and on the very same spot each time, namely, at the distance of one inch from the immoveable bridge.

A MOST CURIOUS AND IMPORTANT EXPERIMENT.

There is a second steel wire, equal in size, which may be placed occasionally on this monochord. The reader may form an idea of the prodigious accuracy of this instrument, from the following experiment, which I have often repeated in the presence of different persons, to the great astonishment of them all.

I begin by stretching the two wires with a degree of tension so precisely equal, that no beating is audible between them. In this experiment, I have generally made the length of each wire exactly twenty inches between the bridges; and each wire has been so stretched as to yield the same sound as that note which is commonly called the first bass G; but any other note would do.

By means of a fine screw, which has fifty threads to the inch, and which has a micrometer head very finely divided on its edge, I can move the slider on one of the bridges to such a minute degree, as to lengthen or shorten that wire as little as *the one hundred and eightieth part of the one hundredth of an inch,* without altering its tension. Such a very small alteration in the length of one of the two wires invariably produces an audible beating between them. And not only the beating may be *heard;* but, what is remarkable, it may likewise be distinctly *felt.* The best way to feel it, is to support a small piece of steel wire, about two inches long, on the soundboard of the monochord, with one of the finger nails. If the lower end of that piece of wire be semi-spherical; if its upper end be pointed; and if that pointed end be applied to the new or tender part of the nail; then, the beating will be *felt* very sensibly.

This beautiful experiment clearly proves how perfectly *unisons* may be tuned; since, *the smallest deviation in an unison* is thus distinctly perceptible. This leads me to my next discovery.

STANHOPE TUNING-GLASSES.

Some persons have had tuning-forks adapted to the pitch of the different notes. One fork, to the pitch of C; another, to the pitch of G, &c. The tuner has then nothing to do but to tune all the Cs of the piano-forte, organ, or other keyed instrument, to the C fork; all the Gs to the G fork, &c. This is perfectly good in theory. But there is, in practice, an objection to forks which is not generally known. It is this. Out of a hundred forks, there is, perhaps, not one which has not a beating in it, when it is struck. How, then, is it possible to tune an instrument accurately by means of forks which do not yield a pure or single sound? It is, however, frequently practicable to get rid of the beating in a tuning-fork, by very carefully filing the two legs so as to make them exactly alike throughout. But, this requires much more attention than is likely to be commonly bestowed.

I have contrived a tuning instrument which is far superior. It consists of thirteen slips of plate-glass; each of which is exactly six inches long, by two inches in breadth. They are tuned respectively one perfect octave higher than the pitch of the keys in the MONOCHORD TABLE, NO. I. For, by varying the thickness, I can tune one slip to C; another, to G, &c. The thickness of the slip which yields the sound of the middle C is about nine hundredths of an inch; and the thickness of the slip which yields the sound of the first treble C is about eighteen hundredths. Those two Cs are the two extremes. Each slip yields a sound which is extremely pure. The same brass support, which terminates in three pointed corks, serves for each slip in succession. Each slip is similarly placed on that support, is struck by the same cork hammer, with the same elastic force, and on the corresponding spot, each time. By means of this simple tuning apparatus, which is pitched to my NEW AND IMPROVED TEMPERAMENT, any careful person, with a moderate ear, can tune an instrument perfectly, and better perhaps than the best tuner could do without this admirable assistance, which to persons in the country may be very useful. And, to professional tuners, this permanent and portable tuning instrument will be extremely convenient.

STEREOTYPED AND PRINTED BY A. WILSON, DUKE STREET, LINCOLN'S INN FIELDS.

XLVIII. *Observations upon Alicant Wine, and particularly the Alicant Raisin Wine.* By M. Pissis, M. D.*

Alicant wine, one of the most precious cordials in pharmacy, has the agreeable colour of red wine added to the mildness of a wine made of must, and it is generally allowed that it has been prepared in this manner. It is evident that the must which produced it had been coloured before fermentation. The only raisin yet known to furnish this kind of must is called the Plant of Alicant; in our country it is known by the name of *teinturier*, or *tachant*. It is bitter, and ripens badly in the centre part of France; but the further we advance to the southward the better are the wines it produces, and the more do they resemble the wines of Spain. I have no hesitation in saying, that with the cultivation and treatment resorted to at Alicant, we may be able to imitate the Spanish wines in the south maritime districts of France.

The colouring matter of the Alicant raisin is the same as that of the red fruits and common red wines; it has the singular property of becoming red by the acids: although blue by nature, it becomes green with the alkalis, and the colour is destroyed by the strong oxygenating substances; but in the common raisin this colouring matter is not dissolved except by alcohol, when the must has fermented: while in the *teinturier*, and the most of the red fruits, it is diluted in their juice. Is the nature of this matter changed in these different fruits, or is it always the same? and is it not, except by means of intermediate substances, resinous or extractive, that this principle is dissolved equally well by the aqueous juices as by alcohol? Several experiments incline me towards the latter opinion: besides, it is more conformable to the simple and constant progress of nature; it is also that of M. Chaptal, who, in his Essay upon Wines, refers the colouring principle of red wine to the fecula.

My object is not to expatiate more upon this principle; I have already described it fully in a letter written to M. Chap-

* *Annales de Chimie*, tome lvii. p. 5.

tal, when minister of the interior, who honoured me with a most flattering approbation of its contents. That letter contains the theory of the deleterious effects of wines and casks, with the most efficacious method of preventing injuries, founded upon authentic experiments collected during these four years. Circumstances have occurred to hinder me from printing this work; but I think it useful to communicate one of the facts which by chance occurred, and which appeared extremely worthy of attention, by its importance to the healing art.

Last year I attended a patient, to whom I frequently administered Alicant wine, which I was perfectly certain was genuine, and it was twenty years old. The patient vomited frequently, and violet-coloured pellicles were seen floating upon the matter vomited: these I took at first sight for the refuse of the coat of the stomach, as I had often seen the like in cases of gangrene in that organ,—with this difference, however, that in the latter case the colour is always more livid: I also remarked that the smell of the former pellicles was acid, and not putrid; they did not appear upon every occasion of vomiting, and they were attended with no other symptom of gangrene.

I concluded from these observations that there must be some chemical illusion in the case. I mixed all the liquids which were administered to the patient with one another; and I at length discovered that these pellicles, so alarming in their appearance, were occasioned by the mixture of Alicant wine with fat broth; and of this all the attendants were also convinced.

The moment the mixture was made the wine lost its colour entirely, and violet-coloured flakes floated on the liquor, like the scrapings of a cask. No other wine produced any such effect. Several kinds, sold for Alicant wines, were also tried without effect. Presuming that the above phænomenon was occasioned by the peculiar nature of the plant, I requested M. Heraud, a good apothecary, and who has the *teinturier* among his vines, to make some experiments, which I was prevented from undertaking myself in consequence of other avocations. M. Heraud accordingly expressed

pressed the must from some Alicant raisins; but this thick and muddy must produced no sensible effects upon soup. M. Heraud then set the must which remained to ferment; and he obtained a fine red wine, very pleasant although not very mellow, and which precipitated the broth as well as the true Alicant wine which M. Heraud had in his shop. It is easy to see that the mucous and saccharine bodies which render the must of Alicant viscous, keep the precipitate suspended, as it happens with ink when strongly gummed; and that fermentation, by destroying a part of these viscous bodies, had rendered the play of the chemical affinities quite free. It is thus that we extract gelatine from quinces and other astringent fruits, without the tannin which they contain forming any precipitate with the gelatine.

The fat broth, well boiled and skimmed, contains, among other principles, gelatine; and as of all the known vegetable matters gelatine is most easily precipitated by tannin, it follows then, of course, that the latter principle exists in Alicant wine and raisins. This tannin serves for the intermedium to the colouring principle, and keeps it dissolved in the water; also the colouring principle forms the greater part of the precipitate; not that the gelatine acts upon it, since it does not act upon the ordinary red wines, but because in this case the kind of colouring principle is united to that of the tannin, its intermedium. The yellow colour of the tannin gives the red wine of Alicant an orange cast. Upon precipitation the colour becomes violet, because the malic acid remains dissolved, and does not redden the colouring principle. The latter would even become much bluer if it did not retain some atoms of tartar.

When the made wine is evaporated, the alcohol carries off with it a part of the malic acid, and the residue becomes so much the more violet-coloured the less tartarous it is. But in the juices of red fruits, and of the *teinturier*, the malic acid, deprived of the intermedium of the alcohol and retained by the saccharine body, remains fixed, in spite of the operations they undergo, and preserves the red colour of these juices.

We may also conclude, that to clarify the wine or the must of Alicant with animal substances, which will discolour them more or less, is attended with inconvenience. The *teinturier* is not the only fruit which contains tannin; several bitter fruits, without doubt, present it to the chemists also; it is in the fermented juices that they discover it by the acid of gelatine.

It is singular that tannin does not give a bad taste to Alicant wine, considering the disagreeable taste which common wine derives from being put into a cask of new oak; but which is easily carried off, as I myself ascertained, by fining down the wine with gelatinous substances. Are there several kinds of tannin which differ in smell? or is the smell of tannin modified in its union with the colouring principle? This last conjecture appears extremely probable. Wine which has fermented in a new oak tub derives no bad taste from it; we cannot deny that there is here plenty of tannin, which is modified, without doubt, by the fermentation, and assimilated to the tannin of the Alicant wine.

Purchasers of Alicant wine will now be in possession of a sure and easy method of ascertaining the quality of the wine sold them as Alicant; a red colour with a slight orange cast, a sweet and spirituous taste with a sharp bitterish flavour, and the property of precipitating gelatine,—all these are the characteristics of genuine Alicant wine. Without the last property in particular, no wine, however good, is genuine Alicant. Boerhaave, in his treatise on nervous diseases, complains loudly of dealers who colour their wines with sumach. Red fruits, and even the *teinturier* itself, are also resorted to for this purpose. Alicant wine is also made in some countries, but the operation is not performed in the Spanish manner. All these wines ought to precipitate gelatine more or less; we must therefore have recourse to attentive tastings, and other proofs already known, in order to discover them.

The well known nature of Alicant wine enables us to ascertain in what diseases it should be resorted to. The tannin it contains places it at the head of the astringent

wines

wines of the antients, so precious in atonic and colliquative fluxes; it is of course necessary to have it of the very best quality. It must not be resorted to in constipations.

Patients of the lower class often request that their wine may be mixed with their soups. It can be attended with no inconvenience to grant them this indulgence; but when Alicant wine is made use of, they ought to be told that a most disgusting beverage results from such a mixture. It is even prudent not to allow fat soups to be taken at the same time with Alicant wine, when treating such as have delicate stomachs. To conclude: the decomposition of Alicant wine in the stomach can have no more bad consequence than all the other wines have upon meeting with the digestive juices; and particularly with the bile, which contains pure soda. Every thing which cannot form part of the chyle necessarily goes into the alvine secretions.

XLIX. *Chemical Observations upon Spathic Iron.* By *M.* Collet Descostils, *Engineer of Mines.*

[Continued from p. 251.]

The results which I have related, in my opinion, prove that the ores of spathic iron vary in their composition, and consequently explain the difference of those obtained by Bergman, Bucholz, and Drappier. Perhaps from new analyses still more variations may be made known.

It would no doubt be very advantageous to be able to distinguish each variety by its exterior characters, because their different compositions ought to have a different treatment in order to obtain the iron from them. The form of the laminæ, and their texture, serves to distinguish spathic iron into two species in the iron-works of Old Dauphiny. The one which is named *maillat* is composed of large flat laminæ; the other, the crystallization of which is confused, and the laminæ rounded, is called *rives*. The first fuses with difficulty; the second, or that with small grains, is, on the contrary, remarkable for its fusibility and for the quality of its pigs, which yield steel easily. The latter is generally white,

white, while that which is made from maillat is generally gray, and yields iron. These two kinds are mixed with advantage in the manufacture of steel.

Perhaps the specific gravity, the loss by calcination, and the alacrity in changing colour, are better proofs of the composition than the texture is. This is a conjecture, however, which can only be verified by a great number of analyses upon specimens of different kinds.

I ought, however, to remark, that the sulphate of magnesia crystallizes nearly similar to the sulphate of zinc, and that this similarity of form probably led Bayen into his mistakes. This chemist, full of the idea that the flakes which he had perceived in the muriatic solution came from the zinc, while they were truly owing to the manganese, must have neglected to examine the crystals he obtained. This opinion is besides confirmed by the details given by M. Dizé, upon the properties of the white earth, from which he only obtained a very few particles of zinc by distillation with charcoal.

It is probably still this same sulphate that M. Sage regarded as sulphate of manganese. In fact, that of magnesia crystallizes easily enough, and before the sulphate of iron.

I have made these observations with a view of concluding from them that magnesia is generally found in greater quantity in the translucid and well crystallized ore, to which the name of *maillat* is given in the department of Isere, and is that also to which the chemists who have been occupied with this substance have given the preference as the purest. The specimen described and analysed by M. Bucholz seems, however, to be an exception to this rule, and I ought to mention it.

Almost all mineralogists regard spathic iron as the most fusible ore of iron, and, in fact, in several countries it is very fusible; but it does not appear that it is every where very easy to melt; and if we reflect on the processes in use in the founderies in which this metal is wrought, we must know that there are but few manufacturers who do not employ additions of different kinds, or particular preparations, for the purpose of rendering it more easily fusible. In some founderies they add to it carbonate of lime; which shows

 that

that lime is not very abundant in the great masses: in others, argillaceous ores are added, and also carbonate of lime. If, besides, we compare the melted produce with the charcoal necessary, we find that this ore requires a considerable quantity of fuel. My colleague, M. Le Livec, engineer of the mines in the departments of Mont Blanc and Leyman, who has made upon this substance very numerous experiments in the department of Mont Blanc, is convinced that, in order to obtain one part of metal, two, three, and even four parts of charcoal are necessary. Besides, the scoriæ always contain globules of metal like small shot: this happens with all ores of difficult fusion.

In some establishments it is found advantageous to expose the ore to the extremes of the atmosphere during a space of time more or less considerable. In some works this exposure takes place after roasting; in others, before and after; and lastly, in some, at the iron-works in Styria for instance, they content themselves with exposure, and do not roast the ore at all. They deposit at the opening of the galleries the ore in beds or in large layers, and leave it exposed to the rain and snow for a long space of time. In a memoir upon the forges of this country, M. Rambourg relates that this exposure continues sometimes fifty or sixty years. In Mont Blanc some forge-masters keep the heaps of roasted mineral always humid by means of a very small stream of water. It has been observed that these different preparations render the mineral much more fusible; and nevertheless in some iron-works the necessity is still felt sometimes of mixing with the above mineral * a certain quantity of the ore called by the workmen *mild ore*, and which comes from the decomposition of the spathic iron, occasioned by a very long action of the air and humidity. In this state the ore, which was formerly

* M. Hericart de Thury, engineer of the mines, in a report to the prefect of the department of Isere, says, in speaking of the veins of the Vaunaveys mountains at Visilles: "White or yellowish translucid carbonated iron is the most abundant. When it has been extracted, it is left some time in the open air to assist its decomposition, and after roasting they expose it again to the atmosphere to render it more easily fusible. In spite of these precautions they are still often obliged to mix it with mild ores (such ores as are entirely decomposed), *so refractory is this spathic ore*."

refractory,

refractory, is extremely fusible, and we may obtain iron from it by the Catalan method with much success.

It is evident that in these treatments even the metals are oxidated, and the sulphuretted iron, which generally accompanies this spathic iron, is converted into sulphate, particularly when roasting is made use of*: this sulphate seems to be taken away by the rains, or by the water which has been passed through the heaps of the mineral; but this separation does not explain the augmentation of fusibility; it seems, on the contrary, that this quality ought to be diminished, because the sulphur is necessarily left in less proportion in the mineral; and we know that this principle renders the iron more fusible. In order to ascertain the true cause of this, let us first inquire into the cause of the refractory property of certain spathic ores.

It is well ascertained that the ores of great laminæ, to which in Dauphinè the name of *maillat* is given, are every where regarded as the most difficult to melt. This ore, as has been said above, is precisely that in which magnesia abounds; and we know that this earth vitrifies with difficulty. Bergman, in his dissertation upon this substance, expressly says, that it does not enter into fusion, except with silex, argil, and lime, or with fluor spar †; and M. Lampadius has made some experiments of the same kind, from which he concludes that magnesia diminishes the fusibility of mixtures into which it is introduced, if it does not destroy it completely.

* I shall say a few words on the subject of roasting.—We know that the ore of spathic iron is found in ridges mixed with quartz, calcareous spar, and sometimes argil, as at Eisen-Arts, according to M. Rambourg, and a quantity of ferruginous pyrites more or less considerable. The ores of Old Dauphiny and Mont Blanc are particularly in these circumstances. The roasting separates a portion of sulphur; it drives off the carbonic acid, which would make the heaps swell up and augment the weight of the ore, and consequently the expense of carriage to the furnace; it lastly destroys the cohesion, and gives the workmen a great facility in separating the quartz and other foreign substances: but one would think that, the pyrites being then confounded with spathic ore by their colour, the workmen could not separate them, and that in time they would effloresce.

† Darcet melted a stone (Briançon chalk) which contained a great proportion of this, by mixing it with gypsum.

In

In order to ascertain if it really was owing to this earth that the refractory properties of spathic ores are owing, I made the following experiment:—I took equal quantities of the ore of Vaunaveys and that of Allevard which I had analysed, and after having pulverized them, and added enough of oil to make a paste, I introduced them into a lined crucible, and I heated them like a specimen of iron. The specimen of Vaunaveys had been a little more heated than that of Allevard, which, as will be recollected, had yielded a very notable quantity of manganese. The latter was perfectly melted, and covered with a green scoria, while the experiment with the ore of Vaunaveys only presented a mass of little coherence, through which was diffused a multitude of small globules of cast iron.

This difference could not arise from the state of oxidation of the iron, since in the specimen of Vaunaveys this metal was reduced into globules; but as it might be supposed that the manganese contained in the specimen of Allevard had some influence on its complete fusion, and that it would not have taken place if it had been exempt from it, I took a new quantity of the ore of Vaunaveys, and, after having taken the magnesia from it by means of the nitric acid, I treated the oxide of iron which remained as I had treated the ore, without continuing the fire so long. I obtained from this trial a button covered with a little brownish scoria; and this button was not only well melted but even a little ductile, and presented in its fracture a gray texture composed of spherical cells like native iron, which, as far as we know, is a first degree of refining in the furnace.

This experiment left me no more doubt upon the cause of the little fusibility of certain spathic ores. In fact, it is evident that the reduction of the oxide of iron is effected with facility. But the metallic globules cannot unite, owing to the earthy particles not becoming sufficiently fluid, and which therefore choke the furnaces. One may still conclude that the ores are more or less fusible according as they contain more or less magnesia; and those which do not contain it at all, and which, on the contrary, contain manganese, ought to be very fusible.

 But

But it remains to ascertain what change takes place by the operation of the air on refractory ores, as well when not subjected to the operation of roasting as when they have been subjected: here I can only offer conjectures; and I can only support them by facts formerly known, and by no direct experiments.

It is well known that spathic iron ores, exposed to the air and to humidity, become brown, and lose their hardness. Their cohesion is almost destroyed. This effect is produced by the oxidation of the metals. The magnesia, which in the ore is combined with carbonic acid, preserves this acid, while its combination with the metals is destroyed by the stronger oxygenation of the latter: thus it ought easily to be carried off by rain, because we know that the carbonate of magnesia is a little soluble; and we should think that during an exposure of sixty years this earthy salt might be carried off almost entirely. Its solubility may, besides, be augmented by the carbonic acid united to the metals, and which is separated from them in proportion as they oxidize.

After roasting, the same explanation will not be applicable; in fact, magnesia should not retain carbonic acid any longer, and it is doubtful that it could have regained enough of it in the atmosphere to become again soluble; but as almost all the spathic ores which have been submitted to this operation contained pyrites, it appears to me that we may explain their amelioration by exposure to the air, by taking in the action exercised on the caustic magnesia which is formed in the mineral by the sulphate of iron. The experiment of Bayen will be recollected, which I have mentioned, and by which he thought he had separated the oxide of zinc from the calcined ore, with the assistance of green vitriol. This action certainly takes effect in the places where the roasted mineral is deposited; and the water, by degrees, takes off the sulphate of magnesia *.

If this cause should appear insufficient, I would observe,

* If there is lime in the ore, and it should happen to be in the caustic state, it would decompose the sulphate of iron in preference; but the experiment of Darcet indicates that the salt which would be formed would contribute to the fusion of magnesia.

that

that all kinds of ores are mixed in roasting, and that the proportion of magnesia in the mass is not always so great as that found by M. Drappier; and that, lastly, it is sometimes necessary to add mild ore to it.

Without doubt, after a long-continued exposure, an ore of easy fusibility might be always obtained; but as this tedious method is attended with very great inconvenience, several processes are in use to shorten the time: the most practicable is to mix some mild ore with the ore of large lamina newly dug; in this case the proportion of the magnesia is endeavoured to be diminished.

Some ores, as well as that of Eisen-Arts, contain quartz, calcareous carbonate, and argil, the most advantageous mixture to promote the fusion of the magnesia; but it is necessary to get rid of the greater part of this earth by exposing to the air the most refractory species of spathic ore.

In some iron-works, also, they mix with the spathic iron carbonate of lime and ochrey ores, which, as is well known, usually contain argil and sand: flux for the magnesia is thus composed.

It is not improbable that the oxide of manganese facilitates fusion a little; at least, by repeating the experiments of Bergman upon the vitrification of magnesia, I think I have observed that this metallic oxide produces a very great fluidity in glass.

To conclude; it is necessary to remark, that all the manganese contained in these ores does not vitrify when it is in great proportion, a part of it being reduced with the iron, and united to the melted metal*. All the trials I made with the specimen of Allevard yielded me buttons of white metal, which contained manganese in great abundance. That of Vaunaveys, and a melting of Vierzon, only afforded some slight traces of manganese in the same circumstances.

The method I employed consisted in dissolving the metal in aqua regia: I afterwards precipitated with ammonia, and heated the precipitate to redness, in a silver crucible, with

* At least in the docimastic experiments.

 caustic

caustic potash. The potash which I employed, melted alone in the same crucible, absolutely remained colourless.

I did not endeavour to determine the quantities with precision, but I am convinced that they were very different.

This observation agrees with the division established by M. Stengel among meltings, and may lead to exact experiments, serving to prove the ingenious conjectures of this metallurgist.

I have endeavoured in this memoir to prove that the ores of spathic iron have not all of them an uniform composition.

That the refractory quality of some among them is owing to the great proportion of magnesia which they contain.

I have endeavoured to explain, by the properties of this earth, the different practices made use of in the iron-works where this kind of ore is melted, and which practices have not been hitherto explained.

I certainly wish that new observations should confirm my conjectures; but if it is proved that I am deceived, I shall nevertheless congratulate myself on having called the attention of metallurgists to one of the most important objects of the science to which they belong.

L. *Description of an Invention for elevating and depressing Water, applicable to the Use of Canal Locks, and for preventing the usual Waste of Water therein. By Mr.* Robert Salmon, *of Woburn*.*

SIR,

For the inspection of the Society of Arts, &c. I have forwarded my model, and you herewith receive inclosed a description thereof. The novelty of this mode of bringing into action a considerable force, will, I hope, appear; and I beg leave to observe, that, besides the principle being applicable to locks, it will apply to many other uses where a lift or descent is required. It may also be right to observe,

* From *Transactions of Society of Arts*, &c. 1806. The society voted Mr. Salmon the silver medal and ten guineas for this improvement.

that

that the curve may be so constructed as to counteract the inclination of the load on the plane under any irregular operation; and, being so constructed, the load will in all cases be nearly as easily moved as if always running on a level surface.

I am, sir,

Your most obedient humble servant,

Woburn, April 23, 1805. ROBERT SALMON.

Charles Taylor, Esq.

In fig. 3. (Plate VIII.) C is supposed to represent a canal lock of the common construction, whose lower gates *i, i*, open towards or into the lock, and its upper gates *k, k*, open towards the upper or higher level of the canal; D is a hollow caisson, or water-tight chest, which is fitted to a walled chamber or side-lock, so as to move freely up and down therein; *i* is an opening, which forms a connection between the lock and the caisson-chamber, and which can be closed by a shuttle fitted thereto, when required. Four standards *e, e, e, e*, are firmly fixed on the ground and walls of the lock and chamber; and four posts *c, c, c, c*, are fixed in the four corners of the caisson; on each alternate pair of these standards and posts the frames *a* and *b* rest, as on so many fulcrums, or moveable joints; the frame *b* (fig. 1 and 2.) has two straight parallel bars of thin iron fixed thereto, and standing up above the same; the frame *a* has two similar bars affixed to it, except that the top edges of these are hollowed into a curve, as shown in the figures. BA is a carriage loaded with two heavy leaden weights, and resting on four low brass wheels, having grooves in their circumferences, like sash-pulleys, to receive the iron bars upon the frames *b* and *a*, so that the carriage can be drawn along upon them; the distance of the axles of their wheels is such, that when the wheels at B rest on the frame over two of the posts *c, c*, the wheels at A shall at the same time rest over the other two posts *c, c*, as shown in fig. 1; and when the wheels at B rest over two of the standards *e, e*, the wheels at A shall at the same time rest over the other two standards *e, e*, in fig. 2. In order to work the model, the

carriage must be brought into the position shown in fig. 1; and this can readily be done by stops, which are provided in the proper places on the curved bars, for preventing the wheels from rolling too far; as much water must then be poured into the lock C, as will fill it exactly to the black line *d, d,* withinside the same; and if the table on which the model stands be not level, small wedges or chips must be put under the model where necessary, until the surface of the water exactly corresponds, all round the lock, with the top water-mark or line above mentioned: it must likewise be observed, to place the model across the table, so that the weight *h,* when hung over the pulley *f* or *g,* may be at liberty to descend. Then hang the two-pound weight *h,* fig. 1, by the line over the pulley *f,* at the upper end of the lock; and the carriage, or load B, A, will be drawn forwards into the position shown at fig. 2, and the water in the lock C will pass through the shuttle to buoy up the caisson D, and its surface in the lock will descend to the lower level. Again, by shifting the weight to the lower end *g,* the load will again be brought back, the caisson depressed, and the water forced through the shuttle, again raised to the higher level *d, d,* in the lock, as in fig. 1.

Hence it is evident that the water in the lock, with or without a boat therein, may be raised or lowered, by the application of any force to move the carriage or load, horizontally on wheels. That when it is intended to pass a boat from the upper to the lower canal, the water in the lock is raised to the top water-level *d, d*; the upper gates *k, k,* are then readily opened, and the boat floated into the lock; this done, and these gates shut, the water and boat, by withdrawing the load from the caisson, is lowered to the lower level of the canal. The lower gates *i, i,* are then opened, and the boat floated from the lock to the lower canal. In this operation of lowering a boat, it is evident, that so far from there being a waste of water, a weight of water equal to the boat and its load is raised from the lower to the upper canal; for when the boat at the upper level first enters the lock, its own weight of water is displaced, and

 forced

forced into the upper canal. And again, when it is floated into the lower canal, as much is again from that canal displaced, and forced into the lock.

On the same principle that water is gained by a descending boat, as above described, it will be observed that no waste ensues in an alternate passage; and that in an ascending passage a loss of water equal to the boat and its load only takes place.

It should be understood that as canals are sometimes more or less full of water, locks on this principle must be constructed to raise and depress, to the greatest extremes that ever happen, from the highest high-water to the lowest low-water mark, and that being so constructed, they will apply to any intermediate heights; the curved plane *a* being formed to adjust and counterbalance the inclination of the wheels on the other plane *b*, thereby maintaining an equilibrium, at any intermediate height, which the water in the canal may happen to be at.

Having described its manner of operating, I shall explain and compare cause and effect; for which purpose it may be requisite first to state, that the load of the carriage B, A, is fifty-six pounds, which weight, when advanced, presses directly over the parts *c, c, c, c,* with all its gravity bearing on the caisson; but when the load is drawn forwards, it rests entirely on the fixed standards *e, e, e, e,* and by this change the whole effect is produced.

Now, if the model be set properly to work, it will be found, that a two-pound weight suspended over the pulley at either end will put the carriage in motion, and thereby raise or depress the water in the lock, and that to do so, the two-pound weight will descend sixteen inches. Hence, two pounds descending sixteen inches may be denoted the cause or power to produce the effect. Further, it follows, that this two-pound weight descending sixteen inches produces the same operation as fifty-six pounds laid in the caisson would perform, and this sinking of the caisson D may be denoted the direct effect produced by the two-pound weight. The indirect and requisite effect being that of depressing or elevating the water in the lock C, and the com-

 parison

parison thereon, will stand thus: the surface of a body of water of an area of twenty-four inches by ten, is raised about four inches and a half by the power of two pounds descending sixteen inches; and, *vice versâ*, by reversing the power, the water is again depressed.

The shuttle *i*, between the lock and the caisson chamber, will regulate the time of the ascent or descent of the caisson.

Woburn, April 23, 1805. R. SALMON.

Charles Taylor, Esq.

SIR,

In reading over the copy of the paper which I hastily drew up, and sent with my model, I observe that I omitted making any remarks on its applicability, improvements to be made in the carriage to facilitate the moving of the load, and on the different other ways, besides the one shown in the model, by which it may be put in action.

It will readily occur to every engineer, that this sort of lock is not confined to the particular shape of the model, or to any particular form. The caisson-chamber may be placed endwise to the lock, may be of any shape, and placed at a nearer or further distance, as may be required.

On comparing the length and movements of the frames in the model with what may be required in practice, it will appear that the length of timbers at large will not be such but that strength sufficient may be obtained for any load. It is also evident, that, although the frames consist of only two bearers in the model, yet, at large, any number may be introduced parallel with each other, and as many wheels as bearers.

In this operation the weight of the carriage itself contributes towards the effect, which in common cases is otherwise, as generally there is an objection to the great weight required to make a carriage sufficiently strong for any extraordinary purpose; and there is no doubt but, by an improvement of the carriage, it may be made to require much less than the power used in the present model. The mode I should pursue would be, to make the load in the wheels themselves; that is to say, the necessary load to produce the effect should be two solid iron cylinders running

on

on as many bearers as are requisite, and to have a frame or carriage for the purpose only of connecting the cylinders; by these means the strength and friction of the axletrees would be reduced very much, and the means required then to perform the operation would be only to put the body in motion, and to overcome any little obstacle or irregularity that the peripheries of the cylinders would meet with in their progress.

The advantage of rollers over wheels has been admitted even where the peripheries of the cylinders were in contact with the incumbent weight resting on the top of them, as well as with the supporting plane below; but in the case above suggested they have more advantage, being only in contact with the upholding frames.

With respect to its operation, if any objections should be found to the great animal power that would at large be required, it will occur, that various other means may be used to put the carriage or load in motion; some without any loss of water, and others with a trifling loss, compared with what the lock holds. Thus, when the caisson is up, if, by a cock, a portion of water be let into it, the equilibrium will be destroyed, the caisson will sink, and the water in the lock be raised. Again, if by a pump, or other means, the water be returned from the caisson to the lock, the caisson will rise, and the load of itself recede, and this would be without waste of water. To put it in motion with a certain portion of waste, it is presumed different ways may be found; as the introduction of a portion of water from the upper canal to the lock, or the discharging of it from the lock to the lower level; these would, with management, occasion the caisson to rise or fall; or, if a part of the load were made to shift further from, or nearer to, the fixed standards *e e e e*, it would thereby cause the action required, and perform the operation; and it is probable that a better way than any here suggested would arise, should the thing be put in practice.

I am, sir,

Your obedient humble servant,

Woburn, May 4, 1805. ROBERT SALMON.

Charles Taylor, Esq.

LI. *Me-*

LI. *Memoir* upon the Decomposition of Water, and of the Bodies which it holds in Solution, by means of Galvanic Electricity.* *By* C. I. T. DE GROTTHIUS †.

CHAP. I.

Action of Galvanic Electricity upon certain Bodies dissolved in Water.

I. WITHOUT wasting time on the discussion of the multitude of imaginary hypotheses invented to explain the decomposition of water by the electrometer apparatus, I shall give a general theory of the decomposition of liquids by Galvanic electricity, which, in my opinion, brings the effects of the latter to a simple and satisfactory explanation. I was led to this theory by the following observations:

II. When a current of Galvanic electricity is made to pass through a saturated metallic solution, the intensity of this current being proportioned to the interval occupied by the liquid, and comprised between the extremities of the two conductor wires, interesting phænomena are discovered even by an observer who does not trouble himself to investigate the cause. At the extremity of the wire in contact with the disk of zinc, oxygen is disengaged; while at the extremity of the wire in contact with the disk of copper, the molecules of the metal in solution are revived, assuming a symmetrical arrangement, which extends in the direction of the Galvanic current.

III. This arrangement is nothing else than an imperfect crystallization of metallic molecules, exactly similar to that known by the name of *arborisation,* and which takes place upon precipitating metals in solution by other metals. The old chemists added to the word *arbor* the name of the deity to whom the metal was consecrated. Thence come the old names of *arbor Dianæ, arbor Martis, arbor Veneris,* &c. Of all the phænomena presented to us by Galvanism, no one is

* From *Annales de Chimie,* tom. lviii. p. 54.

† This memoir was printed at Rome in 1805. We presume the perusal of it here will give pleasure to our readers, as the author himself has also requested us to reprint it.—*Note of French editor.*

is so fine or so interesting as this vegetation, presenting to our view the image of a fine shrub, furnished with its foliage and adorned with the most beautiful metallic brilliance.

IV. Wollaston, the celebrated English chemist, has already noticed, that upon establishing a current of electricity in the solution of a metal, the latter is revived at the extremity of the conductor endued with negative electricity; but I am ignorant whether or no he also perceived that this revival is susceptible of assuming a symmetrical arrangement, when the action has enough of energy and has lasted a sufficient time.

V. All the metals in solution are not equally decomposed by Galvanic electricity. From nitrate of manganese I obtained gaseous bubbles at the negative pole* in place of a metallic deposit; and it seems that when in similar circumstances, the metal, in solution has more affinity for oxygen than hydrogen has for this principle, it is the water which alone suffers the decomposition.

VI. During the arborisation of the metals at the negative pole, no gas is seen to be disengaged; whence I conclude, either that the hydrogen arising is combined with the oxygen of the metallic oxide, or that the action is only exercised upon this oxide and not upon the water. This last conclusion ought to be a true one; for we can scarcely admit that the hydrogen is able to carry off completely the oxygen from the oxides of zinc and iron, as well as from certain acids their solvents, in which these two metals are not dissolved, except after having produced an effect contrary to this admission, by decomposing water.

VII. Of all the metallic salts which I submitted to the action of the electrometer apparatus, the acetate of lead and the muriate of tin † presented the most beautiful vegetation.

* I shall in future make use of the phrase *positive pole* to express the extremity of the wire communicating with the disk of zinc, and of the term *negative pole* to express the extremity of the wire in contact with the disk of copper.

† I have also obtained effects more or less remarkable from the nitro-muriates of gold and platina, from the nitrates of zinc, copper, mercury, and cobalt; from the sulphates of zinc and iron, from the stannite of muriated potash, and from muriate of iron.

That

That of lead imitates the appearance of fern leaves; and upon the ramifications of tin I have often seen, by means of the magnifying-glass, octaëdral crystals. It is remarkable that the arborisation is always directed from the negative pole towards the positive pole, whatever is the position of the two poles, and it is consequently always established in the track of the electrical current. The vegetation of a metal, with the assistance of electricity, seems to imitate, in some measure, that of the natural plants, which constantly incline towards the light, disengaging oxygen by coming in contact with the solar rays.

VIII. When the metallic tree has extended within a short distance of the positive pole its increase is stopped, because its foliage, being infinitely slender in every respect, annihilates the electrical action by exercising the power of an infinity of points. It even seems, that by the too near approach of the poles, each may acquire the electric fluid from the other; for the extremities of the metallic ramifications have sometimes begun to oxidate while deoxidation was manifested in the positive pole. It is probable that always when the extremities of the two conductor wires become very slender and very nearly approach each other in the water, the gases coming from their decomposition are mixed one with another. Here we have, if I am not mistaken, an analogy between the decomposition of water by an electrical machine, and that which is effected by the pile of Volta *.

IX. When the current of Galvanic electricity acts upon water either pure or when charged with some soluble substance, the positive pole attracts the *oxygenating* principle, while the negative pole attracts the *oxygenated* principle of the liquid. If the proportion of the components of the latter is variable, it becomes oxygenated at the extremity of the wire in communication with the disk of zinc, and deoxygenated at the extremity of the wire in contact with the disk of copper. The following are the proofs of this fact:

* Wollaston, upon decomposing water by the electricity of an ordinary machine, constantly observed that the oxygen and the hydrogen were disengaged both at once, while the action of the pile inclines them to show themselves separately.

X. The

X. The muriatic acid becomes so much oxygenated at the positive pole that it acquires the faculty of dissolving the gold coming from the extremity of the conductor wire. The sulphuric and the nitric acids become transparent, and appear to be so surcharged with oxygen in the part surrounding that pole, that I think they are capable of producing effects, when in this state, with which we are not as yet acquainted *. At the negative pole the muriatic disengages a good deal of gas †, the sulphuric acid sends forth a strong sulphureous smell by depositing sulphur, and the nitric acid becomes the nitrous by assuming a blue colour. If the position of the two poles is afterwards changed, so that the one occupies the place of the other, every particle of the acid returns, by little and little, to its primitive state, and the effects recommence.

XI. A solution of muriate of tin, traversed by the Galvanic current, precipitates, by little and little, a white powder, coming from the positive pole. This precipitate, redissolved in the muriatic acid and then tried with corrosive sublimate, altered the latter into white, whereas the liquid which had surrounded the negative pole altered it into black. The muriate of tin had thus become more oxygenated at the extremity of the wire, which excited the liberation of the oxygen.

XII. After a long action of the Galvanic electricity upon the sulphate of iron in solution, the latter becomes turbid, assuming a red colour in the part which surrounded the positive pole. We may ascertain that it then contains a strongly oxidated sulphate of oxygenated iron by trying it with the prussiate of potash, which immediately produces a very fine Prussian blue with this part of the liquid, whereas that which surrounds the negative pole only produces with the same prussiate a precipitate of a greenish white colour.

XIII. The molybdic acid dissolved in the concentrated

* At this degree of oxygenation the sulphuric acid seems susceptible of dissolving gold; at least, that which I made use of in this experiment assumed a yellow colour in proportion as it dissolved the extremity of the gold wire which liberated the oxygen. On pouring into this sulphate of gold a solution of green sulphate of iron, a precipitate was formed like the sulphuret of gold.

† It would be interesting to examine if this gas comes partly from the decomposition of the acid.

sulphuric

sulphuric acid, assumes, in the cold, a fine blue colour, which always disappears when heat is applied to the solution. Upon exposing it to the action of the pile of Volta, the vitreous fluid acts in a manner analogous to heat, while the resinous fluid produces an effect analogous to cold; on the positive pole the liquor becomes by degrees perfectly transparent, and the molybdic acid is partly precipitated in the form of a white powder, whereas round the negative pole it always acquires a deeper and dirtier colour. Upon afterwards changing the position of the two poles, so that the one occupies the place of the other, the contrary happens; the transparent part returns to the blue colour, and the blue part becomes transparent.

XIV. When the Galvanic current exercises its influence for a long time upon the solution of an earthy salt, the base of the latter is gradually precipitated round the extremity of the wire of negative electricity. These precipitates, in my opinion, are not the effect of a decomposition by the alkali which is generated at this point in an infinitely small quantity; but I presume that the acid of the salt is there destroyed, or well decomposed, whence it results that its earthy base becomes free.

The glass tubes containing the solutions submitted to the experiments I have here described, were often covered with a metallic crust, which seems as if melted upon the vitreous matter of the interior of the tube, and which comes from the particles of metal detached by the action of the apparatus of the conductor wires: thus when these extremities were of gold or silver, the glass tubes became perfectly gilded either with the one or the other metal.

Chap. II.

Theory of the Decomposition of Liquids by means of Galvanic Electricity.

XV. The decomposition of water by the electrometer apparatus has for a long time exercised the ingenuity of chemists and naturalists, to whom this phænomenon affords a delicate problem to resolve in order to reconcile it with the theory of the nature of water. It is first necessary to know if

if the two products of the two Galvanic poles come from one and the same molecule of water, or rather from two different molecules; and in the latter case we may ask what becomes of the hydrogen at the place where oxygen only is perceived? and in return, what becomes of the oxygen where hydrogen only is perceived?

XVI. The column of Volta, which will immortalize his name, is an electrical magnet, every element of which (*i. e.* each pair of disks) possesses its negative and positive pole. The consideration of this polarity suggested to me the idea that it might establish a similar polarity among the elementary molecules of the water solicited by the same electrical agent; and I confess that this afforded me a spark of light on the subject.

XVII. Let us suppose, therefore, that at the moment of the generation of the hydrogen and the oxygen, there takes place in these two bodies, as well by contact as by the friction of the one against the other, a separation of their natural electricity in such a manner that the former acquires the positive and the latter the negative state; it follows, that the pole from which the resinous electricity continually flows will attract the hydrogen by rejecting the oxygen, whereas the pole animated with the vitreous electricity will attract the oxygen by rejecting the hydrogen *. Thus, when the Galvanic current traverses a quantity of water, each of the two component principles of the latter is solicited by an attractive force and by a repulsive force, of which the centres of action are reciprocally opposite, and which, by acting in the same manner, determines the decomposition of this liquid.

XVIII. The action of each force, in respect to a molecule of water situated in the direction of the Galvanic current, is in the inverse ratio of the square of the distance to which it exercises its influence. But as the distance of any given molecule placed between the two centres of action can never

* Considering the diversity of substances deposited at the negative pole, it would be more simple, and perhaps more just, to admit only an attractive and repulsive force acting upon the oxygen, without attributing it to the poles in relation to the hydrogen.

diminish

diminish relatively to the one, without increasing itself on account of this diminution relatively to the other, so each of the two elements of such a molecule is solicited by a constant force, which results from the attractive and the repulsive force *.

The effect of the repulsion, although effectively existing, is not sensible, on account of the reciprocal action of the elementary molecules in contact, whence results a recombination of such as are repelled by the Galvanic poles.

XIX. Let us consider, however, a certain quantity of water, composed of oxygen, represented by the negative sign (—), and of hydrogen marked by the positive sign (+). See + fig. 1. Plate IX. At the moment of establishing a current of Galvanic electricity in this water, the electrical polarity manifests itself among its elementary molecules in such a manner, that the latter seem to constitute the complement of the pile in action. At the same time all the molecules of oxygen situated in the track of the current will have a tendency to make their way towards the positive pole, whereas all the molecules of hydrogen situated in the same track will tend towards the negative pole.

It thence results, that when the molecule of water represented by o,h, yields its oxygen o to the vitreous fluid of the wire +, its hydrogen h is immediately reoxygenated by the arrival of another molecule of oxygen o, the hydrogen of which, h, is recombined with r, and so on. The same thing takes place, but in a contrary sense, relatively to the molecule of water Q, P, which, on yielding its hydrogen Q, to the resinous fluid of the — wire, is immediately rehydrogenated by the arrival of the molecule X; and this succession of decomposition and of recombination of the elements of the water will continue until the latter is completely decomposed.

XX. It is clear that in the whole operation the molecules of water, situated at the extremities of the conductor wires, will alone be decomposed, whereas all those placed interme-

* I suppose that each force has the same intensity; a circumstance which ought actually to take place, since neither of the poles of the electrometer apparatus can acquire electricity except at the expense of the other.

 diately

mediately will change reciprocally and alternatively their component principles without changing their nature. From this I infer, that if it were possible to establish a current of Galvanic electricity in water, in such a manner as to describe in the latter a perfectly circular line, all the molecules of the liquid situated in this circle would be decomposed and instantly recomposed: whence it follows, that this water, although undergoing the effect of the Galvanic action, will always remain water.

XXI. Having exposed some liquids, contained in two or more vases, to the action of the electrometer apparatus, I perceived the polarity at the extremities of the metallic wires which serve to establish the communication between the liquids inclosed in each vase. (See fig. 2.) Thus, when the vases contained acetate of lead dissolved in water, I obtained oxygen at the extremities *a* and *c*, while the vegetations already described rose at the extremities *b* and *d**.

Upon bringing the vessels nearer together, and upon curtailing the dimensions of the wire *b c* as much as possible, the electrical polarity was nevertheless distinctly perceptible; by imagining the same wire *infinitely small*, one may conceive how the molecules *n* and *p* unite upon regenerating the body which was at first decomposed.

XXII. The theory of the decomposition of water here given leads us to the following consequences:

(a) The proportion of hydrogen could not have increased in the part of the water which is nearest to the positive pole, since the oxygen of the whole quantity of the liquid traversed by the Galvanic current inclines towards this point, while the hydrogen endeavours to recede from it.

(b) An oxygenation in that part of the water which surrounds the negative pole is equally impossible, since the hydrogen is there constantly attracted, while the oxygen is repelled from it. See § IX.

* I have communicated my memoir to M. Morichini. This chemist informed me that he obtained an analogous result upon examining the gases which are liberated when the gases only contain water. The extremities *a* and *c* yielded oxygen gas, while the hydrogen gas came from the extremities *b* and *d*.

(c) When

(c) When even the component principles of water are not susceptible of any other proportion of combination than of that which makes it water, the latter would not be less decomposed in the manner described; but there would neither be oxygenation, nor hydrogenation, nor acidity, nor alkalinity, in any part.

XXIII. The production of an acid at the positive pole, and that of an alkali at the negative *, urged by Galvanic electricity, is also a support to the theory proposed; for, according to analogy, we ought to attribute the former to an oxygenation, and the other to the presence of hydrogen †. See § IX.

My apparatus having remained several days in action, the cloths moistened with a solution of muriate of soda were here and there covered with a saline efflorescence, which was nothing else than soda united to carbonic acid which it had absorbed from the air.

XXIV. The polar arrangement, such as exists in the elementary molecules of water traversed by the Galvanic current, ought to be established equally among the elementary molecules of every other liquid body, provided they are solicited by the same forces. In the metallic solutions the electric polarity takes place among the elements of the oxide, the oxygen of which passes to the positive pole, and the metal of it is deposited at the negative pole. The acid reacts upon these metallic particles which it holds in solution; but being decomposed, as well by this re-action as by the electrical power, the revival does not the less take place.

XXV. I filled a bent tube with two different metallic solutions, in such a manner that each of them, without being mixed with the other, occupied one of the moieties of the

* Tincture of turnsole, traversed by the Galvanic current, becomes red around the positive pole, and returns to the blue colour upon changing the respective position of the two poles; but these effects may be explained by the action of the oxygen and the hydrogen at the moment of their production upon the colouring matter, and are not sufficient to deduce from it the acidity and alkalinity.

† Hydrogen is a constituent part of volatile alkali, and oxygen enters into the composition of all the acids with the nature of which we are acquainted.

 tube,

tube, and they had a simple point of contact in the middle*. On exposing the two liquids thus arranged to the action of the Galvanic current, and on plunging the *negative pole* both into the one and the other, it was always covered with revived metal in whatever solution it was plunged.

If we knew any other substance besides oxygen which may be acted upon by the positive pole, we might repeat this experiment relatively to the latter. An analogous result would then evidently prove that the decomposition of water by Galvanic electricity takes place in relation to two different molecules; an opinion generally admitted, and conformable to the theory which I have now submitted to the examination of the learned.

The admirable simplicity of the law to which this phænomenon is submitted, coincides, to our astonishment, with the laws of the universe. Nature can neither *create* nor *destroy*; since the number of bodies is never augmented nor diminished, but all without exception are subject to a mutual exchange of their elements: and when we consider the wonderful effects of electricity, which acts often in secret, although spread over the universe, we cannot refrain from pronouncing it to be one of the most powerful agents of the grand operations of Nature.

LII. *On the Imitation of Marble and Plaster Figures by a new Composition made of old Paper reduced to Paste* †.

John Nicholas Gardeur, an artist of Paris, has invented the method of imitating the most beautiful sculptures by means of old paper reduced to paste; it is also ascertained that this new species of ornament adds, to a wonderful lightness and solidity, the requisite truth in the expression of the figures.

M. Gardeur is the first who attempted, with success, this new branch of industry; and almost all the theatres and public

* We may easily perform this operation if we make use of two solutions of different colours; for instance acetate of lead and nitrate of copper.

† From *Biblioth. Phys. Econ.* for December 1805.

public halls in Paris are decorated with statues, &c. of his composition.

These productions are as cheap as common painted paper, and from their lightness may be transported to a great distance without much expense. The encouragement given to M. Gardeur by the French government, determined him to exert himself in an extraordinary manner to bring his invention to perfection, and he has now so far succeeded as to have nothing left to wish for; he has extended his industry so far as to render his figures capable of ornamenting the outside of buildings also, by rendering them impenetrable by the weather.

The members of the National Institute who were appointed by the minister of the interior to examine the performances of M. Gardeur have declared, that to the best of their knowledge he is the only artist who ever exercised that branch of manufacture, and that they were perfectly satisfied with the execution of the specimens shown them on their visit to his manufactory.

LIII. *Report made by M.* Guyton *upon a sculptured Head in Flint, with a Covering of Chalcedony. Read in the French Institute on the* 31*st of March* 1806.

M. Millin, a member of the class of antient history and literature, having had occasion to examine, as an object of antiquity, a piece of sculpture found at Roule, thought proper to submit it to the inspection of the class of Physical and Mathematical Sciences, as being interesting both to mineralogy and the processes of the arts; and Messrs. Berthollet, Vauquelin, and myself, were charged with making a report on the subject.

This curiosity was obligingly lent us by M. Cerf, the gentleman to whom it belongs. It had been found, four months ago, in the garden of a house which forms part of the chateau of Fernes, in the suburb of Roule, at present occupied as a boarding-school. It was discovered by a gardener, at about five or six decimetres below the surface.

This

This is all the information we were able to collect on the spot, nor has any thing been discovered which could lead to the least conjecture upon the epoch or circumstances of its being buried below ground; but the singularities which it presents sufficiently excite the attention of the antiquary, the naturalist, and the artist, to induce them to gratify their curiosity by an examination of what remains of it.

It is a head sculptured upon a piece of flint of the same kind as that of which gun-flints are made. From the lower extremity of the chin to the summit of the cranium it measures 3 inches 4 lines; from the forehead to the occiput, 76 millimetres; its circumference, taken above the nose, is 236 millimetres.

A hole of 13 millimetres in diameter, made in the lower part, is still filled up with plaster mixed with lime, and seems to have served to unite the head with the body of the figure, the latter part being probably of silex also, or of some other material more easy to be wrought upon, and which, according to the usual proportions, may have been about 54 centimetres: so that the whole statue may have been about 63 centimetres, or 23 inches 4 lines in length.

The head-dress indicates a male figure; the hair is short, and confined by a simple narrow bandage, as worn by the Greeks and Romans; a circumstance which, joined to the style of the figure, seems to ascribe to this work a date far remoter than the time of the Gauls, although the eye-balls are strongly marked in it, which is very seldom seen in any true antique monument.

But we leave to more competent judges the discussion of these points, which it appears only necessary in us merely to mention in order to complete the description of the monument, and to present in its true light the question which has chiefly excited the attention of the Institute.

The flint of which this head is formed has been covered, wherever it was not broken or rubbed, with a coating of a fine white, of a thickness scarcely perceptible; it was attacked by none of the acids, and it united to a hardness at least equal to that of chalcedony, the vitreous consistence

of an enamel sufficiently translucid to admit of the different gray or blue shades of the silex being seen through it.

Is this covering (for I do not think I can give it the name of incrustation) a work of nature or a production of art?

One would think that analysis might be resorted to in order to resolve this question; but the article would be entirely destroyed, and even by destroying it we could not get enough of this covering matter from which to obtain unequivocal results, and after all, nothing more might be learnt than is already known from its external characters of colour, opacity, hardness, and inalterability in the acids; *i. e.* that its constituent parts are the same as those of chalcedony.

The first idea presented by the inspection of this head is, that the block of silex, after having been laboriously sculptured with a drill, like other hard stones, had received a covering in the fire of the nature of that applied to the biscuit porcelain. It was not only the gloss of its enamel, and its thinness, that seemed to found this opinion, it was still more confirmed by the comparison of its lustre with the roughness of the white crust of the two fractures at the bottom of the left cheek, a crust evidently formed since its being deposited in the ground.

But a large fracture, more recent, discovers the silex preserved with all its ordinary characters; and we know that this substance loses its colour and transparence in a fire so low as to be incapable of fusing feldspar. The piece which underwent this experiment was only exposed to a heat of 13 pyrometric degrees; it was divided into several fragments, and it assumed the appearance of a biscuit in the interior.

This certainly strengthens the idea, that the chalcedony which covers this silex could only have been placed there by the humid way, while it was in the ground.

Before adopting it, I thought proper to inquire among the collections of minerals of this kind for indications, at least, of the possibility of the natural production of a similar covering.

Flints are generally seen incrusted rather than covered

vered with this production; the crust is unpolished and rough to the tongue, imbibes the acids, and even sometimes gives signs of effervescence. There are, to be sure, some of them covered with a very hard chalcedony, but always thicker, less transparent, forming an unequal crust, presenting only some brilliant particles in the fractures, where we may perceive the traces of the friction, but never of the gloss of enamel.

One would think that the polish received by flint ought to account for the gloss of chalcedony which covers it, and produce the difference of the earthy chalcedony which covers the two fractures; but the chalcedony observed upon several rock crystals, upon mamellated agates, or that which we meet with upon cubes of fluate of lime, or on other crystals, the united surfaces of which may be considered as polished, never presents any thing like a shining aspect.

The stalagmite chalcedony of Geyser, in Iceland *, also has no appearance of enamel even upon the surfaces which have been in contact with flat bodies. The hydrophane chalcedony, such as we observe forming passages in pechstones, petro-silex, &c., are also of a rough white, often even in recent fractures; besides, they are always found in veins, and never in the form of a crust. It may be said of them, as well as of opals, that their fracture, although of a brighter lustre, is always unequal and undulating, and presents nothing to the eye approaching the lustre which polishing gives them.

Two pieces, however, presented to me a surface polished enough to encourage the hope of finding the analogy of this silex in nature. One of these pieces came from the department of the Indre and the Loire; it appeared entirely covered with white chalcedony; but having broken it, in order to examine the interior, I saw nothing but a mass of the same nature, which had no polish on its surface, except what it acquired by rubbing; a circumstance which excludes all connection with the former.

The other piece came from Siberia; one of its surfaces

* See Bergman's xiiith Dissertation on Quartzy Earths.

approached a little more to a glazed enamel, and had a pretty lively lustre; but this mineral, also completely different from common flint, was only an opake white chalcedony upon a more hyaline chalcedony. It was moreover intersected by reddish-coloured lines, which crossed in different ways, in the manner of the *ludus*.

The doubts excited in my mind by these comparisons engaged me to examine if the subject of investigation could not have been the work of art, or at least how nearly it approached it.

I have already remarked how easily silex is altered by the fire; it is not necessary, therefore, to have recourse to the processes of coating porcelain to solve our difficulties. But might not the same end be attained by cementations at a moderate heat, long digestions in saline fusions, or in combined solutions, in order to bring into play efficacious affinities?—Chemical experiments can alone throw light on the subject.

It will be sufficient cursorily to explain the results of the first fruitless trials.

Silex, cemented in the lime of marble, sulphate of lime, sulphate of alumine, and in the muriate of soda, experienced no alteration so long as the heat was not pushed beyond a certain degree, after passing which it begins to lose its colour, transparency, and tenacity.

A fragment of silex, treated in caustic potash in a platina crucible, only experienced a slight diminution of weight, more or less considerable according to the time that it was kept in a heat capable of maintaining the potash in fusion.

Alumine being (although in a small proportion) one of the constituent parts of chalcedony, I thought that by treating silex in a solution of potash saturated with alumine, and adding to it a quantity of free potash to act upon the silex, the well-known affinity of the two earths for each other, and with a common solvent, would operate a new combination on the surface of the silex, at a degree of heat incapable of causing any other alteration.

Considering, on the other hand, that an analysis had ascertained the presence of lime in some chalcedonies, I put

put a little of the former, with silex, in the same preparation of potash and alumine.

These two experiments were made in crucibles of platina; and the success exceeded my hopes, although they had not been preceded by any preliminary trial in order to regulate the doses of the agents or the duration and intensity of the fire. The silex was not altered in the interior; it only assumed at its surface a very slender white covering, of an uniform thickness, forming one body with the mass, not attackable by the acids, and of such a hardness that it soon wore out the stones made use of by the engravers; and it was no more affected by the application of adamantine spar, or corindon, to it, than was the covering of the sculptured head.

These pieces came out of the crucible dull white, as I expected; but some parts, which I wrought in the manner of hard stones, showed that they were susceptible of the same polish as that of the sculptured head.

It cannot be denied that such a perfect imitation is favourable to the opinion that the covering of the head was the work of art. We are not, however, entitled to suppose that the chemical affinities which led to this imitation were known to the artist who executed this monument; it is not the first process discovered by accident long before the discovery of the true theory.

This opinion, however, has not obtained general assent. Those who oppose it chiefly support themselves upon the resemblance of the coverings of several silices found in the environs of Fernes, specimens of which have been presented to the class by M. Chaptal, and which in fact present upon some of their surfaces particles of enamel, if not equal in colour and thickness, at least as shining.

Others are of opinion, with M. Fourcroy, that whether the covering of the sculptured head had been formed in the earth in the same manner as the crust of these silices, or whether they had been added after the labour of the sculptor by a process of art; yet, in all cases it must be acknowledged that it had received the polish from the hand of man, and this is the only method of reconciling the results which we are forced to draw from its actual state.

In

In these circumstances the judgment of the public must be still suspended, and the road remains open to discussions and researches, in order to resolve a question equally interesting to the history of the arts, the sciences, the antiquary, and the naturalist.

Explanation of Plate X.

The flint head, with the covering of chalcedony, is represented at three-fourths of its natural height.

It is seen in fig. 1. on the left side, in order to make visible the recent fracture *a*, which discovers the common flint with its usual characters.

In fig. 2. it is represented on the right side; the fractures *b* and *c* are the two antient ones, which during the continuance of the sculpture in the earth were covered with a rough and dull chalcedony.

LIV. *Report of Surgical Patients admitted into the Finsbury Dispensary, from the* 1*st of April to the* 31*st of August*, 1806. *By* John Taunton, *Esq. Surgeon to the City and Finsbury Dispensaries, and Lecturer on Anatomy, Physiology, and Surgery.*

In the last surgical report (see Philosophical Magazine, April 1806,) there were 86 patients under cure; 53 of whom have been cured, 15 relieved, 5 not known, and 13 still remain on the books.

Since the above report, there have been admitted into this dispensary 287 patients.

Cured	-	-	145
Relieved	-	-	19
Irregular	-	-	1
Under cure	-	-	119
Not known	-	-	3
			287

Many

Many of the above persons have had ulcers situated on the lower extremities, in which more pain than is usual has been experienced, and the cure protracted beyond what might have been expected.

Suppuration of the *inguinal* and other glands has been frequent, unconnected with the lues venerea or scrophula, which have been successfully treated by the application of stimulating cataplasmas, with tonics taken internally.

Many cases of diseased mammæ have occurred; as abscess, scirrhi, and cancer: in the treatment of scirrhi much apparent good has been derived from the administration of the ferri rubigo, with stimulants applied externally, as mentioned in a former report.

Mrs. R——, aged 47, Cow-Cross, has been several years subject to femoral hernia, which seldom remained down long; and she could return it at any time previous to the present attack by placing her body in a horizontal situation. It came down on Saturday the 19th day of April, and could not be reduced. The pain became considerable over the fore-part of the abdomen, attended with sickness, and occasionally with hiccup. These symptoms continued to increase till the 25th, four P. M., when I was requested to see her.

The hernia was small, and situated on the right side; the pulse 120, thready, and irregular; respiration short and oppressed; skin dry; the tongue brown: the pain was referred particularly to the region of the *stomach*, from which every thing was rejected as soon as taken: the hiccup and vomiting having now become extremely troublesome, and as several attempts had been made to reduce the hernia without effect, it was thought right to administer the following injection:

℞. Nicotiana ʒj. aq. bullient. lb. j. f. enema.

This produced great lassitude; but the hernia could not be returned during its operation; and of the following pills two were directed to be taken every two hours:

℞. Calom. gr. x extr. colocynth. ʒß. f. pil. vi.

These were immediately rejected, and the vomiting continued to increase during the night. In the morning the pulse was 130;

130; feculent matter vomited, and every bad symptom had increased, so as not to leave any hope of success, except in the performance of an operation; which was no sooner proposed than acceded to, and borne with the greatest fortitude. Immediately on the reduction being accomplished, she expressed herself comfortable, and in three hours after she had a copious evacuation; the pulse 116, with a gentle perspiration diffused over the whole body.

℞. Tinct. opii gtt. xl. aq. menth. ℥ß. M. f. haust. Statim sumend.

On the 27th she appeared quite well, had slept during the night, and expressed a desire to have something to eat; the pulse 120, but soft.

℞. Aq. ammoniæ acet. aq. menth. āā ℥ iv. M. cochl. ij. bis in die sumend.

28th, she had had a comfortable night, and appeared to be recovering fast, being desirous to sit up.

29th, the bowels had been more loose, attended with pinching pain for the last eighteen hours.

℞. Opii gr. v. rhei gr. xxx. f. pil. x. capt. j. bis in die.

30th, she was much better, and sat up several hours; the wound was dressed which had nearly healed, from which time she continued to regain her health; and in a few days came to the dispensary, being perfectly cured.

J. F. aged 68, Green Arbour Court, Old Bailey, glass-cutter, has had a femoral hernia on the right side for several years, which he has always been enabled to reduce by pressure when in bed. It came down on Wednesday, the 23d of July, and could not be returned; he became sick immediately; an enema was administered without any relief being obtained, and cathartics appeared to increase the complaint. 26th, the stomach was become extremely irritable, every thing being rejected as soon as taken, and the hiccup was particularly distressing; the pain and sense of heat in the umbilical region became almost intolerable. These symptoms continued to increase, and on Monday, the 28th, in the afternoon I was requested to see him. All attempts to reduce the hernia were ineffectual; the countenance was truly hippocratic; the pulse intermitted about every fourth beat;

beat; respiration was much oppressed, cough very troublesome, and the tongue was covered with a brown fur. The operation was immediately performed; on which the sickness left him, and copious evacuations were produced in two hours.

℞. Tinct. opii gtt. xl. aq. puræ ℥j. M. f. haust. —Statim sumend.

He rested well during the night; and in the morning expressed himself to be comfortable, excepting the inconvenience which he experienced from the hiccup, which had returned and was very troublesome.

℞. Aq. am. acet. aq. menth. āā ℥ iv. M. capiat coch. ii. ter in die.

30th, pulse 80, skin soft; a gentle perspiration was diffused over the whole body, but the hiccup was almost constant and very distressing; the dressing, having become loose, was removed, and the wound had healed at the upper part, and looked much better than could have been expected. 31st, the hiccup continued; but appeared to have been more troublesome at three o'clock in the morning and at eleven o'clock in the forenoon than at any other time during the twenty-four hours. The same observation was made on the preceding day.

℞. Aq. menth. pip. ℥ viii. æth. vitr. ʒii. tinct. opii ʒß. M. coch. ii. omnia secund. hora sumend.

August 1st, the hiccup had entirely ceased, and every unfavourable symptom had disappeared: he requested to go abroad, which was not permitted; the wound was dressed, which had nearly healed.

3d, he continued to improve in strength, walked out daily, and in a few days was enabled to resume his labour, and was discharged cured.

In the above cases the operation was performed under the most unfavourable circumstances; the patients being extremely poor, not having conveniencies, or even the common necessaries of life. It may be asked, Why were they not sent to an hospital, where every necessary convenience and attention would have been afforded, by which, apparently, they would have had a much greater chance of recovery?

very? but experience militates against this opinion; for I have invariably seen, that patients recover in less time from the most severe operations when in the bosom of their families, even in distress, than when taken to a hospital where all apparent inconvenience is removed. But in hernia this fact is particularly illustrated, and may be referred to two causes: 1st, the state of the patient's mind in leaving his family; and 2dly, the time in which the operation is performed, as patients in general cannot be prevailed on to leave their home for an hospital till they are in the greatest danger; and as the success of this operation depends on its early performance, all delay beyond the necessary attempts at reduction tends to increase the danger. "I have often had occasion to lament that I had performed the operation too late, but never that I had performed it too soon *."

It is much to be regretted, that a small fund is not provided and annexed to all charitable institutions for the purpose of supplying the poor with trusses, by which many lives would be saved, and a painful operation frequently prevented. This might be done at a very inconsiderable expense, comparatively speaking with the benefit that would accrue to society from its adoption; as the same truss could be procured for seven shillings for which the patient is obliged to pay a guinea or even a guinea-and-a-half, to the great distress of his family; and which in the majority of cases is totally out of his power to spare, be the danger ever so great.

It is my intention to resume this subject in a subsequent report, and to make some observations on the hernia which sometimes takes place in the omentum and in the mesentery.

Greville Street, Hatton Garden,
Sept. 16, 1806.

* Mr. Hey's (of Leeds) Observations on Surgery.

LV. *Extract of a Memoir upon Hair. Read at the French National Institute, by* M. VAUQUELIN *.

THE principal object which the author had in view on undertaking his experiments on the above subject, was to ascertain the nature of the animal matter of which hairs are formed, and if there was any thing analogous in the animal œconomy. But in the course of his experiments phænomena presented themselves which, appearing foreign to the principal substance, led him further than he intended: it did not enter into his plan at first to inquire into the cause of the various colours of hair, which nevertheless became the principal object with which he was occupied. It is only, he says, after labouring a long time upon the same object, by carefully observing the phænomena which arise, and by meditating on the causes which produced them, that we arrive at results often impossible to foresee *à priori*. Nevertheless, he does not flatter himself that he has penetrated into all the secrets of nature on this subject; nor does he propose his ideas but with that reserve which ought to be shown in such difficult researches. He gives, however, an exact description of his experiments; compares and discusses them, and draws such conclusions as appear to him the most natural. We shall now give an abridgment of the chief of his experiments, as well as the corollaries he deduces from them.

I boiled, says he, some hair in water for several days, without being able to dissolve it; the water, however, contained a small quantity of animal matter, as was demonstrated on the application of infusion of galls and other reagents.

It is probable that this matter, which gives the water the property of putrefying, is foreign to the proper substance of hairs. I conclude from this experiment, that at the temperature at which water boils in the air, hairs cannot be dissolved.

* From *Annales de Chimie*, tome lviii. p. 41. Messrs. Chevruil and Caballe, two of M. Vauquelin's pupils, assisted at the experiments related in the above memoir.

I suc-

I succeeded in dissolving them without alteration in Papin's digester, by regulating the heat properly. If in this operation a certain degree of temperature is exceeded, the substance of the hair is decomposed in whole or in part; a circumstance demonstrated by the ammonia, carbonic acid, and the fetid empyreumatic oil found in the solution, to which the oil communicates a deep yellow colour.

In the one or the other case, sulphureted hydrogen gas is liberated in great quantity, which acted strongly upon the copper of the digester, which it blackened: more was found when the heat was raised; which seems to indicate that this substance is produced during the operation.

If we operate upon black hair, or if the heat is not sufficient to decompose it, there remains a black matter, which, on account of its minute division and the consistency of the solution, is deposited very slowly. This matter is principally composed of a black oil, as thick as bitumen, little soluble in alcohol and the alkalis of iron and sulphur perhaps united with each other. Red hair leaves a yellowish red residue, containing plenty of oil, sulphur, and a little iron.

The solutions are almost entirely colourless when they are filtered; the concentrated acids make them turbid, the weak acids produce no change: an excess of these agents restores the liquor its former transparency. Tincture of galls and the oxygenated muriatic acid form abundant precipitates. Silver is blackened in it; the acetate of lead is precipitated from it brown. These solutions, evaporated with every necessary precaution, were not jellied, and only furnished a viscous and gluey matter; whence I concluded that the substance of hair is not gelatinous.

The acids form precipitates more abundant and higher coloured in a solution of hair made at a higher temperature, for this reason, that they decompose an ammoniacal soap, which does not take place in the former case.

I likewise dissolved black and red hair in water containing only four per cent. of caustic potash. During this solution hydrosulphuret of ammonia is liberated: which seems to announce a commencement of decomposition in black hair,

hair, leaving a black residue formed of thick oil, a little more animalized, and of iron and sulphur. There remains, after the solution of red hair, a yellow oil, containing sulphur and an atom of iron.

The acids form in these solutions white precipitates, soluble in an excess of the above menstrua. These precipitates being redissolved in the acids, there appeared upon the liquor, after some time, an oil under the form of a prismatic-coloured pellicle.

The solution of hair in potash precipitates lead of a black colour, owing to the hydrosulphuret it contains; that of red hair appears to contain more of it. When they are freed from the sulphur by exposure to the air, they have merely the smell of soap, and become frothy in the same manner.

The acids act upon hair each in their own manner: the sulphuric acid and the muriatic acid assume at first a very fine rose colour, and afterwards dissolve it. The nitric acid makes it yellow, and dissolves it also by means of a gentle heat: the solution presents at its surface a black oil when black hair is used, and a red oil when red hair is made use of. Both the one and the other of these oils become white after some time, and become concrete by cold.

This same solution evaporated properly yields a good deal of oxalic acid, and the uncrystallizable mother-water contains a bitter substance, plenty of iron, and sulphuric acid coming from the sulphur of the hair.

The solution of red hair in the nitric acid contains less iron, but more sulphuric acid than that of black hair.

The oxygenated muriatic acid gas whitens hair at first, afterwards softens it, and reduces it to the form of a viscous and transparent paste like turpentine. This matter is bitter; it is partly dissolved in water and partly in alcohol.

I obtained from hair submitted to the fire in a close apparatus the same products as from every other animal matter, with this difference; that it furnished more sulphur and yielded but very little gas: it left in the retort from 28 to 30 per cent. of charcoal.

By incineration hair furnished iron and manganese, which

 gave

gave a brownish yellow colour to the ashes, phosphate, sulphate, and carbonate of lime, a little muriate of soda, and a considerable quantity of silex. The ashes of red hair are less coloured, because they contain less iron and manganese; those of white hair contain less also, but abundance of magnesia is found in them relatively to the other principles. Hair leaves little more than 1·5 of ashes.

Alcohol extracts two kinds of oil from black hair; the one white, which is deposited upon cooling in the form of small brilliant flakes; the other, which separates in proportion as the alcohol volatilizes, is of a greenish gray, and also becomes concrete in time.

Red hair also yields a white and concrete oil like spermaceti; but alcohol leaves a deposit by evaporation of another oil, which is as red as blood. What is remarkable and interesting in this experiment is, that the reddest hairs upon which alcohol was employed became brown or deep chestnut-coloured. I conclude from this, that the colour of red hair is owing to the presence of this oil.

According to the experiments reported in this memoir of M. Vauquelin, (a great many of which we have omitted, as being only accessary to the principal object,) it appears that black hair is formed of nine different substances, viz.

1. An animal matter, forming the greatest proportion.
2. A white concrete oil, in small quantity.
3. Another greenish gray oil, very abundant.
4. Iron, the state of which in hair is as yet uncertain.
5. Some particles of oxide of manganese.
6. Phosphate of lime.
7. Carbonate of lime, in a very small quantity.
8. Silex, in a notable quantity.
9. Lastly, a considerable quantity of sulphur.

The same experiments ascertained that red hair does not differ from black, except that it contains a red oil in place of a greenish-black one; finally, that white hair differs from the two former, in so far as its oil is almost colourless, and it contains phosphate of magnesia, which is not found in the others.

According to this knowledge of the nature of the constituent

tuent principles of hair, M. Vauquelin thinks that the various colours which distinguish this substance may be accounted for. The black colour, according to him, is produced by a black and bituminous-like oil, and perhaps also by the combination of sulphur with iron. The red and flaxen colours are owing to the presence of a red or yellow oil; the intensity of which diminished by a small quantity of brown oil makes the hair red. Lastly, the white is owing to the absence of the black oil and sulphuretted iron. He thinks, that in red and yellow hair, as well as in white, there is always an excess of sulphur; since when the white metallic oxides are applied to it, such as those of mercury, lead, bismuth, &c., they become black very speedily. The manner in which this body acts upon metallic substances makes him think that it is united to hydrogen.

M. Vauquelin endeavours afterwards to explain the whiteness which suddenly takes place in the hair of such persons as are struck with profound grief, or surprised by great fear. It must be supposed, he says, in order to explain this phænomenon, that at the moment when nature is in a state of revolution, and when consequently the natural functions are suspended or changed in their nature, an agent is developed in the animal œconomy, which, passing into the hair, decomposes the colouring matter of it.

But what agent can produce this effect? The acids appear to him to be alone capable of it: this at least is certain, that black hair plunged some time in these menstrua, and particularly in the oxygenated muriatic acid, becomes white very sensibly.

The rapid production of an acid in the animal œconomy does not appear impossible to him, upon considering that a movement of passion in men, as well as in animals, is sufficient to change the nature of certain of their humours and render them poisonous; and seeing that the Galvanic fluid often determines in animal and vegetable matters the formation of an acid or an alkali, according to circumstances. As to the whiteness which hair gradually undergoes from age, he attributes it to the want of secretion of the colouring matter.

Independently of the animal matter which forms the

 basis,

basis, there is in hair a colouring matter which may be separated from it, and the shade of it varies according to the colour it shows. It is to this fat substance that M. Vauquelin attributes the pliability, the elasticity, and the inalterability of hair. It is certainly also to the same substance that they owe the property of burning so rapidly, and forming soap so abundantly with the alkalis.

After having treated the colouring matter of hair, he endeavours to characterize the animal substance which forms it, by comparing it with all those already known. Without detailing all the experiments he made with this view, we shall only say that it was not gelatine; since the solution in water, which is done with much trouble, never becomes a jelly after evaporation: neither is it albumen; because it cannot be dissolved in boiling water without being decomposed, and the solutions act differently with the reagents.

The humour which the substance of hair approaches most, if not absolutely the same, is, according to M. Vauquelin, that which physiologists denominate mucus, or animal mucilage, which is neither gelatine nor albumen.

This humour, which is separated in the nostrils, the mouth, the œsophagus, the tracheal artery, the stomach, the bladder, and in general in all the cavities of the body, gives a great deal of viscosity to water, and also the property of frothing very strongly by agitation. In certain *coriza* it becomes thready, like the substance of silk or spiders' webs; preserves its transparency and flexibility after desiccation; and M. Vauquelin does not doubt that if it contained a little oil, it would completely resemble the substance of hair.

The epidermis, the nails, corns, wool, and down in general are formed of the same animal mucus, and equally contain in their composition a certain quantity of oil, which gives them their elasticity and pliability.

The beginning of some experiments undertaken by M. Vauquelin upon the humour of the *plica polonica*, which was furnished him by M. Alibert, physician of the hospital of St. Louis, induced him to think that it was of the same nature as the substance of hair; and that it is what is superabundant to the formation of the latter.

LVI. *Thirty-first Communication from Dr.* THORNTON, *relative to Pneumatic Medicine.*

To Mr. Tilloch.

No. 138, Leadenhall Street,
Sept. 20, 1806.

DEAR SIR,

I HAVE the honour to inclose you the following very interesting case, proving the efficacy of the vital air.

Case of Loss of Voice cured by vital Air.

Miss Norton, a very amiable young lady, daughter of a cutler in Fish-Street Hill, London Bridge, from a severe cold lost the use of her voice; and notwithstanding much medical assistance, she continued in that state upwards of a year and a half. She could not be heard except by the closest attention; and spoke as in the lowest whisper. Her general health was somewhat impaired, but her lungs seemed unaffected. I ordered her bark, and zinc, with valerian, and the inhalation of the vital air, a gallon a-day, diluted with five of common air. This plan was continued a month without any sensible alteration. No warmth was felt after the inhalation, no alteration as to appetite; in short, no sensible difference, and the *aphonia* continued as before the application. I requested, therefore, to see the young lady inhale the super-oxygenated air myself. By mistaking the orders delivered for using the pneumatic apparatus, I found she had only been inhaling common air, and I pointed out to her the mistake. The same medicines were continued; and at once the benefit was perceptible to every one. In a few days the voice recovered its powers; the appetite was increased; a peculiar glow was felt; and in a fortnight this lady was completely restored, and has since remained cured upwards of six months.

Observations on this Case by Dr. Thornton.

1. The loss of voice is a disease that usually resists all common means made use of; therefore, here the *vital air* is a *desideratum*.

 2. The

2. The mode of cure is probably by strengthening those muscles which give tone to the voice.

3. Hence, in the more oxygenated climate of England, the *nightingale* has so melodious a strain.

4. And probably for the same reason this poetic songster only *croaks* in Egypt, into which country it retires in the winter.

5. Several of my patients have observed a remarkable strength of voice arising after inhaling the vital air.

6. A decided experiment was made on this subject by a *pneumometer* invented by me, by which the capacity of the lungs may be ascertained: trying with this instrument the capacity of the chest of the right hon. Charles James Fox, Mr. Courtenay, Mr. Kemble, sir James Mackintosh, Bryan Edwards, &c., the power of voice was found to be in exact proportion to the quantity of air inhaled, and therefore probably to the oxygenation of the muscles of the larynx.

7. Mrs. Siddons made the following decided experiment. After inhaling a super-oxygenated air, she stationed a lady in a remote part of the boxes; and upon the days the vital air was inhaled only, all her lower tones were *most distinctly* heard, and I noticed myself this remarkable effect.

I remain, dear sir,

sincerely yours,

ROBERT JOHN THORNTON.

LVII. *Letter from* MARC TAERG, *Esq. of Beeston, near Shrewsbury, to Mr.* TILLOCH, *on the Preparation of Composts for Land, and on the Composition of Potash.*

SIR, Beeston, September 1806.

HAVING for some time been engaged in making experiments to ascertain what composts are most advantageous for my land, I send you this letter for insertion in the Philosophical Magazine, hoping that it may be of some use to farmers, and throw light upon a subject which has lately attracted much attention.

I will not now take up your time by relating the various mixtures

mixtures made use of in my experiments; suffice it to say, that lime and dung, properly blended and fermented, proved to be by far the most fertilizing of all that I tried.

Having ascertained this fact, I was desirous to know what proportion of lime was best to be mixed with the dung. I found, after various trials, that the best was one-fourth by weight; the largest quantity of nitre being produced when the two were mixed in this proportion.

To hasten the process of fermentation, I made my experiments in a hot-house, kept at a proper temperature, through which a current of air was allowed to pass. When the fermentation was over I analysed the residue; and I constantly found that part of the lime had disappeared, and that the nitre formed was always nearly in the proportion of two and one-twelfth to one of the lime missing.

From the results of my experiments I was led to conclude, that potass takes about two-thirds of lime into its composition. The remaining one-third I suspect is oxygen, for reasons of which I will inform you in my next, in which you will receive an accurate detail of my experiments.

LVIII. *Memoir upon Ultra-marine. By Messrs.* DESORMES *and* CLEMENT. *Read in the French Institute January* 27, 1806*.

THE superb blue colour known by the name of *ultra-marine* has not as yet been an object of chemical inquiry: chemists have hitherto only made experiments upon lapis lazuli, which is, in some measure, the ore of ultra-marine, and it is never found in the state of a crystal; one specimen only excepted, in the possession of M. Guyton.

A process is employed to extract ultra-marine from its matrix which seems to have nothing analogous, and of the theory upon which it is founded we are entirely ignorant: it consists in mixing well, pulverised lazuli, with a melted mastic, composed of resinous pitch, wax, and linseed oil. When

* From the *Annales de Chimie*, tom. lvii. p. 317.

 this

this mixture is well made it is allowed to cool, and then bruised, or ground with a pestle or roller in warm water. This water soon gets dirty; it is then thrown away and new water made use of, which soon assumes a very fine blue colour: when it is sufficiently saturated it is allowed to stand some time, and then several other washings are taken from the lapis lazuli, until the last water is no longer of a blue colour. These washings afterwards deposit a blue powder, beautiful in proportion to the richness of the lazuli. The matrix of the ultra-marine remains blended with the mastic.

In our researches we employed ultra-marine of various qualities; but that which was the foundation of our estimate of the proportions of its constituent parts, was the most beautiful we could procure; we only got it at the rate of two or three per cent. from a beautiful lazuli: nevertheless it was by no means completely pure, but it was at least fifteen or twenty times more so than the lazuli from which it was procured.

The following are the results of our labours:

1st, The specific gravity of ultra-marine is to that of water : : 2360 : 1000.

2d, This substance, such as the process yields, contains oily or resinous matters, which are decomposed by fire; their charcoal is completely burnt by the contact of the air; the ultra-marine becomes red, and upon cooling resumes its former fine colour: in this operation it loses a little of its quality, and it is only by means of attrition that it is brought back to this state of fineness and softness which it had at first.

3d, In a more violent heat, which may be to 1500 degrees of the centigrade thermometer, the ultra-marine melts into a black enamel, if the mastic which is mixed with it is not completely burnt; and it melts into transparent glass, almost colourless, if the mastic is completely burnt. In this fusion it loses 12 per cent. of its weight.

4th, Treated in the fire with borax, it easily gives a very transparent glass; sulphur is liberated, and a little carbonic acid, the quantity of which varies according to the quality of the ultra-marine.

5th, Ex-

5th, Exposed to the action of the electrical pile, the oxygenating side discolours it completely; the hydrogenating side occasions no change.

6th, Oxygen gas alters the colour of ultra-marine exposed to a red heat, it makes it turn to a dirty green; there is an augmentation in weight of one per cent., probably owing to sulphuric acid which is formed and fixed.

7th, Hydrogen gas, in the same circumstances as oxygen, changes completely the colour of ultra-marine; it gives it a reddish colour, and takes off its sulphur; no water seems to be formed, although there is a loss of weight exceeding a little that of the sulphur.

8th, Sulphur in fusion does not discolour it, and after volatilization the ultra-marine is as beautiful as ever.

9th, Liquid sulphurated hydrogen has no action upon it at all.

10th, Neither has lime water.

11th, Barytes water discolours it when heated, and is afterwards found to contain silex and alumine.

12th, The sulphuric, nitric, muriatic, and oxymuriatic acids suddenly discolour ultra-marine: the three former, when concentrated, form with it a very thick jelly; the latter acid dissolves it almost entirely.

If the sulphuric and muriatic acids are diluted in water, there is a liberation of sulphurated hydrogen; the action of the nitric acid produces nitrous gas and sulphuric acid.

13th, The acetous acid acts like the above acids, but much more feebly.

14th, Potash and soda in solution, heated with ultra-marine, diminish its weight; they then contain alumine. The colour is not altered.

If pure potash is heated strongly with ultra-marine, its colour is destroyed; the result of the fusion is reddish, and acts almost as if the ultra-marine was argil, or a stone composed of silex and alumine.

15th, Ammonia has no action at all upon this substance.

16th, Upon heating oil with ultra-marine, the weight of the latter is found to be diminished after washing it in an alkaline solution.

17th, The

17th, The analysis of ultra-marine appears to us more difficult than that of a stone composed in an analogous manner, although it is very attackable by the acids and alkalis. The disunion of its principles is not complete until the most decisive action of each of the reagents generally employed.

The quality of the ultra-marine we employed (which we could not regard as perfectly pure), and the variation which we found in its constituent principles, determined us to study their nature better than their quantities. We assigned a distinct portion of ultra-marine to the investigation of each of these principles, and, after uniting our results, we came to the conclusion that 100 parts of ultra-marine are composed of,

Silex - -	35·8
Alumine - -	34·8
Soda - -	23·2
Sulphur - -	3·1
Carbonated lime -	3·1
	100·0

We always experienced losses of about five per cent., and sometimes more.

The carbonated lime which we found is not essential to the composition of ultra-marine, no more than the iron met with in ultra-marine of the first quality, produced from lazulite a little saturated with sulphurated lime. We do not even always meet with sulphur in this substance.

The following is the manner in which we recognised the nature of the four substances, which to us appear to be essential to ultra-marine:

Thirty grammes (one ounce) of fine ultra-marine, heated with sulphuric acid, left a residue weighing 14·0. The liquor evaporated presented some crystals of alum *, and plenty of sulphate of soda in long needles.

* It is probable that the alkali which had caused this sulphate of alumine to crystallize was potash arising from the ultra-marine; we do not, however, assert this as a fact, because we could not protect this salt from the ammoniacal vapours which might be in the laboratory.

All

All these crystals, and the liquor remaining, yielded, by means of ammonia, 6·85 of dry alumine, and melted in the fire 9·60 of sulphate of soda.

We have seen, by other experiments, that the alumine and soda were generally in greater quantity than that indicated by the action of the sulphuric acid.

On passing muriatic acid gas through water in which 20 grammes of ultra-marine was incessantly agitated for some time, we dissolved 18·48. The remainder, being 1·52, had all the characters of silex. We obtained from the solution 4·6 of dry alumine; muriate of soda, containing about four grammes of alkali; and lastly, sulphate of barytes, containing six decigrammes of sulphur, supposing it composed of $\frac{33}{100}$dths of sulphuric acid, and the latter of $\frac{52}{100}$dths of sulphur.

If with alcohol we treat the result of the fusion of twenty grammes of potash with five grammes of ultra-marine, the weight of the latter is diminished one gramme, and the alcohol contains but very little silex and alumine; this loss is evidently owing to the soda of the ultra-marine, which abandons the other principles, because their combination was broken by the potash in the fire.

Upon treating of ultra-marine by carbonated soda, we got from 10 grammes 3·3 of silex, which had all its proper qualities in a less equivocal degree than it had formerly presented, when arising from ultra-marine treated by the acids or caustic alkalis. We thought at first that it contained some foreign substance, but we could not discover any. In order to characterize this silex properly we employed the usual methods; among others, volatilization by the fluoric acid, which left it deposited like a jelly in the water which it had passed through.

Thus ultra-marine yields on being decomposed, silex, alumine, soda, and sulphur.

Let it be recollected that this precious substance, such as the present process of extracting it affords, contains oily particles; that soda is one of its elements; and it may be added, that the water, which was made use of to separate the ultra-marine from the mastic with which it was incorporated

in

in the ore, was soft to the touch, like an alkaline ley; it gave, on evaporation, an alkaline residue, and we may easily deduce from it the following theory:

The mastic is mixed with the lazuli to combine oil with ultra-marine in order to form a kind of soap, which warm water takes away, rendering it a little soluble; while the matrix remains entangled in the mastic, from which it cannot be dissolved so easily as the ultra-marine, because it wants soda, and consequently cannot separate, like ultra-marine, from the fat resinous substance, which forms a kind of net for it. In a word, the process of extraction of ultra-marine is a true *savonnage* (ley-making);—let the awkwardness of this expression be excused on account of its convenience.

We have here given what appears to us may be concluded from our experiments without hazarding too much. May this first experiment, upon a substance equally little known as it is singular, be followed by its artificial production!

LIX. *Galvanic Experiments by Mr.* D. Gardner, *Lecturer on Chemistry at the City Dispensary.*

To Mr. Tilloch.

DEAR SIR,

In the course of performing a few galvanic experiments, I was induced to try the effect of that fluid on some of the vegetable infusions.

Turmeric in distilled water was the first submitted to trial; about six drachms in a glass tube containing two bright iron wires, forming the circuit between two batteries containing twenty-five plates of six inches square surface. Gas was given out from both wires, and the infusion became gradually changed from a bright yellow to a deep brown, beginning at the upper part of the tube; both wires became black, most probably from their combination with the oxygen evolved from the water. The same quantity of the infusion of litmus in distilled water was subjected in the same manner to the galvanic action: in a few minutes the blue tinge began to fade, the liquor became more diaphanous, and at length exhibited

exhibited a greenish colour, gas being given out from both wires, which also were turned black.

These two experiments led me strongly to conceive that an alkali must have been formed during the operations, seeing that alkalis produce precisely the same effects : to prove this, I restored the blue colour of the litmus by a few drops of dilute sulphuric acid, thinking that the acid would combine with the alkali formed ; which was the case : it was again submitted to galvanic action, which effected a change of the blue colour, and the production of a green as before.

Taking a fresh quantity recently made, I changed its colour to red with the above acid, and, on galvanizing it in the same manner, had the pleasing satisfaction to find that the red became very soon blue in consequence of a production of alkali which must have saturated the acid.

Having repeated these experiments several times with the same results, I was fully satisfied as to the formation of an alkali ; but wanting more proof, to a portion of the infusion of litmus, which had been turned green, I added a few drops of the sulphate of iron, a slight precipitation of which in the state of a red oxide took place : this is always the case when an uncombined alkali is present. Syrup of violets diluted with an equal quantity of distilled water, and galvanized with silver or iron wires, turned as perfectly green as it could have done on the addition of pure ammonia, potash, or soda. If these facts are in the least new or interesting, by their insertion in your valuable magazine you will much oblige, Yours respectfully,

D. GARDNER.

City Dispensary,
Grocer's-Hall Court, Poultry,
20th Sept. 1806.

LX. *Letter from Dr.* DELAVILLE *to M.* VAUQUELIN, *upon the Oxidation of Metals, and particularly of Lead*.*

I HAVE been some time in the habit of pursuing, as often as my other occupations would permit, some experiments

* From *Annales de Chimie*, tom. lviii. p. 52.

upon

upon the oxidation of metals, and particularly that of lead; and as the results I obtained were of a nature sufficiently encouraging to induce me to proceed in my researches, I request that, if you are of the same opinion, you will give that degree of publicity to them you may think proper.

We know that upon rinsing a glass bottle with a little water and lead (small shot), the friction soon detaches small portions of the lead, which, being suspended in the water, give it at first a slate colour; if the agitation is a little longer continued, the water becomes grayish, and a little longer still, it becomes whitish, until at last it is of a very fine white.

This oxide of lead has such a tendency to unite with carbonic acid, that when exposed to the open air, upon being taken out of the water, it becomes almost immediately covered with a pellicle of a brilliant whiteness, which seems to be nothing else than carbonate of lead.

This oxide of lead remains always under water without experiencing any sensible alteration, either in the shade or in the light; but if, when it is inclosed in a crystal bottle full of atmospheric air, and containing a small quantity of water, there is any of it sticking to the inside of the bottle above the level of the water, and if the bottle is exposed to the rays of the sun, in this case the portion of oxide exposed to the light will be seen to pass successively to the yellow, and from the yellow to the red, and thus furnish masticot and minium.

This white oxide exposed in a glass capsule to the heat of a fire, soon passes from the white to the yellow, and from the yellow to the red, in the same manner as that which was exposed to the light.

This oxide may be made in great quantity, and at a small expense, and on this account render some preparations less costly in which it may be employed; it may be also made use of in painting.

The following is the method I had recourse to in order to obtain this oxide: I inclosed a quantity of lead (small shot) in a leaden barrel, into which I poured about one fifth of water; I allowed the rest of the capacity of the barrel to be filled with atmospheric air, and I turned the barrel round by means

means of an axis adjusted at its two extremities. Every one knows how easy it is to turn round a machine of this kind by means of a water-mill.

In order to procure a continual supply of fresh air in the barrel, I contrived to introduce at various points in its circumference leaden tubes open at both their extremities, and which almost met in the centre of the barrel in the form of radii; the other end of these tubes protruded a few inches from the outside of the barrel, to which they were soldered at their junction with it so closely as not to admit of any water escaping.

You will find in the box I have sent you a little bottle, numbered 1., containing oxide of lead, prepared as above, and which has never yet been taken out of the water.

In No. 2. there are pieces of oxide dried in the air in a glass capsule; the brilliant white surface of it was exposed to the air of a room during its desiccation, and the porous and dirty white surface was that which was in contact with the glass of the capsule.

In No. 3. there is the same oxide hanging to the end of the tube, and become yellow, then red, by simple exposure to the light of the sun. The tube is sealed hermetically, and filled partly with water and partly with atmospheric air.

In No. 4. is the same oxide, hanging in a tube hermetically sealed, also filled partly with water and partly with air, exposed for a very long time to the light on one side, but coated on the other with a mixture of wax and lamp-black.

No. 5. is a small bottle, containing oxide of lead agitated with water and carbonic acid gas.

If you find the present results worthy your attention, I shall communicate to you the further progress of my experiments.

I have given the name of *oxide* to No. 1. I think it is an oxide, but it does not appear to be a common one: I would have examined it with more attention, but it required more time than I can at present spare.

LXI. *Ex-*

LXI. *Extract of a Letter to Professor* Pictet, *from a Correspondent at Munich, upon some galvanico-magnetic Experiments recently made by M.* Ritter *.

31st December, 1805.

At the last sitting of the academy on the 16th instant, M. Ritter communicated the first part of a series of experiments intended to ascertain the nature of magnetism; having been led to the subject by the questions proposed by our academy in 1776 and 1777. He begins by establishing the analogy between magnetism and electricity. The principal results of his experiments, of which I have often been an eye witness, are as follow:

1. That each magnet or needle may be regarded as equivalent in point of electrical relation to a pair of heterogeneous associated metals. The different poles respectively represent the two dissimilar metals.

2. Consequently each magnet, like these metals, produces electricity. One of the poles gives positive electricity, and the other negative electricity.

3. A circle of magnets also constitutes in analogous circumstances a battery of Volta; and in this manner the author succeeds in demonstrating, by means of the electrometer, the electricities produced by the poles of this circle of magnets.

4. This battery of magnets exercises upon living bodies, or such as are recently dead, on account of its strength, the same effects as a Voltaic column of equal strength.

5. These experiments demonstrate, that in iron magnetized, the south pole yields positive electricity, and the north pole the negative. On the contrary, in the magnetized steel, the north pole yields positive electricity, and the south pole yields negative.

The same inverse distribution is observed in the oxidabilities (modified by magnetization) of the magnetized body. In iron submitted to this operation, the south pole is the most oxidable, and the north pole is the least. In magnetized

* From *Bibliothèque Britannique*, vol. xxxi.

steel,

steel, it is on the contrary the north pole which is the most oxidable, and the south pole the least.

M. Ritter concludes his report with conjectures upon the application of his results to the earth, considered as one great magnet. In this he finds the explanation of several phænomena, chiefly of the physical diversity of the two hemispheres and of the auroræ borealis and australis.

According to these principles, the earth considered as a magnet may be represented as equivalent to a Voltaic column of an enormous size, in which the poles are on one hand in continual communication by the intermedium of the waters of the ocean; while on the other the superabundant electric matter, finding no conductor, flies off into the free spaces of the heavens, and there produces the polar aurora.

LXII. *Proceedings of Learned Societies.*

PHARMACEUTIC SOCIETY OF PARIS.

The above society has announced two prizes, the one a gold medal of 200 franks in value for the best essay, and the other of 100 franks in value for the next in merit, on either of the nine following questions:

1. Does there exist a process for constantly obtaining kermes of the same colour and nature?

What are the causes of the differences presented by kermes, prepared several times successively after the very same process?

2. How comes it that whey does not always clarify of the same colour? Why does it sometimes redden, and sometimes render green the blue colours? And to what cause ought we to attribute its faculty of keeping a longer time in vessels of tin than in those of clay?

3. Which is the best process for obtaining the purest and most energetic emetic?

What change does the emetic tartar, or its solution in pure water, undergo through time, either on account of the action of light, air, or heat?

 What

What are the alterations which the above salt undergoes in the various vehicles employed to administer it in, and particularly lemonade, orange-water, whey, &c. &c.?

4. What is the difference between electuaries recently prepared, and those which are several years old?

5. Which is the best method of preserving the various parts of plants, in regard to their aroma, colour, the time of gathering, their alterability, and the method of preserving them from insects?

6. Endeavour to ascertain the natural families of the vegetables from their chemical properties; *i. e.* determine the principles which occasion the differences between them.

7. Point out the best method of preparing distilled waters from aromatic plants, or such as are said to be inodorous either in B. M. or in the open fire.

Which of these waters ought to be prepared with plants previously carefully dried?—and is drying a good method of preserving them a long time without alteration?

8. Which are the extracts that ought to be prepared from green, and which from dry plants, by maceration, infusion, decoction, or evaporation?

9. The products of the infusion or decoction of inodorous substances not being the same, determine wherein the difference consists, compare the two methods of analysis upon vegetable matters which do not contain volatile principles. Thus, for instance, we know that an infusion of coffee differs very much from coffee a long time boiled; we know also, that meat-broth is more gelatinous when obtained by slow ebullition than by a hasty boiling; lastly, we know that the extracts of plants become partly insoluble by means of the prolonged action of boiling water. Give the theory of these phænomena.

The members of the Pharmaceutic Society themselves are excluded from obtaining the above prizes; but every foreigner whatever may write for them.

The various essays must be transmitted on or before the 1st of December, 1806, to M. Bouillon Lagrange, secretary to the society, at Paris.

Each essay must have a motto or device, and must be accompanied

accompanied with a sealed paper, containing the same device, with the name and address of the author. This paper will only be opened in the event of the memoir to which it belongs being found to merit one of the prizes.

FRENCH NATIONAL INSTITUTE.

At the meeting of the above learned society, on the 7th of July last, M. Biot read an essay on the changes occasioned in bodies by the action of light; and count Rumford read a treatise on the adhesion of the particles of water to each other.

SOCIETY OF ARTS AND SCIENCES OF HAERLEM.

The new king of Holland has undertaken the presidency of the above society, and in future its title is to be "The Royal Society of Haerlem."

LXIII. *Intelligence and Miscellaneous Articles.*

BLEACHING BY THE MURIATIC ACID.

Doubts having been started on the advantages of this process, the following publication appeared on the subject in the *Moniteur* of the 16th of May, 1806.

Messrs. Descroizilles, bleachers, after Berthollet's method, at Rouen, having been consulted by the prefect of the Lower Seine department, were desired to found their report upon the opinion of one of the most celebrated calico-printers in Europe, M. Oberkampf. The result of their application to M. Oberkampf, as contained in the answer of M. Widmer, his nephew, who directs the manufactories, was; that for these fifteen years past, M. Oberkampf has constantly used this process for bleaching linens and cottons, both mixed and unmixed, and that they always assumed much stronger colours than when bleached by the old process. "It is true," says M. Widmer, "that there are many pieces of cloth in the market extremely ill bleached; England in particular

ticular has exported the greatest quantity of such cloth. The fault does not lie with the process, but arises from the ignorance of some bleachers, who have made use of the oxygenated muriatic acid without knowing how to appreciate its effects. When the stuffs have been incompletely bleached by this process, they labour under the serious inconvenience of retaining a wine tint after being maddered. Let bleachers acquire some notions of chemistry, and these inconveniences will soon give way to the superior advantages of the Bertholian process.

Messrs. Descroizilles add, that care must be taken not to employ soap in bleaching cloths intended for printing. Cloths well bleached by any process, but finally soaped, will never receive a perfect impression or maddering, unless, before they are printed, they be cleaned with great care from the oil left by the soap. It cannot be taken off by the most careful washing, and, it absolutely requires new alkaline leys.

Thus the discovery of M. Berthollet, when properly employed and without the interference of soap, certainly deserves to be preferred to the old process, which requires much more trouble, and besides is not practicable in winter.

MARTIAL ETHER.

M. Tromsdorf has maintained sulphuric ether, when digested over red oxide of iron, to be a very good medicinal preparation as a tonic and antispasmodic. M Cadet, a chemist of Paris, has endeavoured to ascertain from his own experience the reality of the action of this fluid upon the metallic oxide; and it results from his experiments, that ether never acts upon the red oxide of iron unless it still contains some acid; and in this case a sulphate of iron is formed, which is generally precipitated in such a manner that the presence of the portion retained by the ether is only manifested by the most sensible reagents.

MISCELLANEOUS.

Mr. Cuthbertson, No. 54, Poland Street, philosophical instrument-maker, and member of the philosophical societies of Holland and Utrecht, has in the press his work on Practical

Practical Electricity and Galvanism; being a translation of the greater part of the instrument experiments contained in a treatise published by him during his residence in Holland, with the addition of all such as have since been invented by himself and others; together with an appendix, containing the most interesting experiments on Galvanism.

On Tuesday, the 2d of September, the Knippenbuhl Rock, which formed the summit of Mount Rosenberg, in the canton of Switz in Switzerland, was suddenly detached, along with a great portion of the mountain. This tremendous body rolled down into the valley, which separates the Lake of Zug from that of Lauwertz, overwhelming four whole villages, and destroying part of several others. Upwards of 1000 persons were killed by this disaster, and only thirty remain alive out of the whole population of the district where it happened.

Lectures.

Mr. Home's Lectures on the principal Operations of Surgery, given gratuitously to the pupils of St. George's Hospital, will commence in October next as usual.

Mr. Gunning will likewise give Lectures on the Lues Venerea.

Mr. Gunning, surgeon extraordinary to his royal highness the duke of Sussex, and surgeon to St. George's Hospital, will commence his Lectures on the Principles and Operations of Surgery on Monday the 13th of October next, at his house, No. 43, Conduit-street, Hanover-square.

Private pupils may be accommodated in Mr. Gunning's house.

Mr. John Taunton, member of the Royal College of Surgeons in London, surgeon to the City and Finsbury Dispensaries, &c. will commence his Autumnal Course of Lectures on Saturday the 4th of October, 1806, at eight o'clock in the evening, precisely, at No. 21, Greville-street, Hatton Garden, to be continued every Tuesday, Thursday, and Saturday, at the same hour.

In this Course of Lectures the structure and functions of the Human Body will be explained; the situation and connection of the different Organs will be demonstrated, and their

their transition from health to disease noticed; which will be illustrated by preparations of the Parts, Drawings, and recent Dissections, for the information of medical students, professional and scientific persons.

Gentlemen desirous of qualifying themselves in a short time, for the army or navy, will receive particular attention.—The mode of conducting surgical treatment will be familiarly explained, by operations on the dead subject.—The Lectures on the Skeleton, separate bones, and muscles, will contain full instruction, and are intended as well for the use of the artist as the professional student.

An ample field for professional instruction will be afforded by the privilege which pupils may enjoy, of attending the Clinical Practice of both the City and Finsbury Dispensaries.

Particulars may be known on application to Mr. Taunton, Greville-street, Hatton Garden.

Mr. D. Gardner will deliver the Introductory Lecture to his Autumnal Course of Chemistry, on Monday evening, the 6th October, 1806, commencing at eight o'clock precisely, at the Paul's Head Tavern, No. 7, Cateaton-street.

These Lectures are intended to exhibit a concise view of the general principles of Chemical Science, which will be illustrated by numerous experiments, and applied to the explanation of the various Phænomena in the Works of Nature, as well as the more elegant and useful Arts of Life, including Pharmacy, as a branch of Medical Science.

The Course will contain from thirty to forty Lectures, which, after the Introduction, will be delivered on Wednesday and Friday evenings, at the City Dispensary, Grocers'-hall-court, Poultry. To begin precisely at eight o'clock. Tickets for the Course, 2*l.* 2*s.*

LIST OF PATENTS FOR NEW INVENTIONS.

For the Months of August and September, 1806.

To Richard Ford, of the city of Bristol, ropemaker; for his new kind of cordage, made by a process entirely new, from old rope, or junk, or such short ends of new rope as are now commonly converted into oakum or coarse paper; by means of which process the objections to cordage usually

usually termed twice-laid cordage, are totally obviated, and the newly invented cordage is made nearly equal to cordage made from new materials. Dated August 30.

To Thomas Pearson, of Haberdasher's Place, in the parish of St. Leonard, Shoreditch, county of Middlesex, whole-sale upholsterer; for his machine or machinery for the purpose of cleansing, seasoning, and dressing feathers and other articles. Dated August 30.

To John Carey, L. D., of Camden-street, Islington, in the county of Middlesex; for his various contrivances for preventing or checking fires, and preserving persons and property therefrom, by means of divers improvements in alarums, chimneys, cisterns, fire-screens, and other articles. Dated August 30.

To Christopher Wilson, of Windmill-street, Tottenham-court-road, in the county of Middlesex, master mariner; for his new system of naval architecture. Dated August 30.

To Robert Newman, of Dartmouth, in the county of Devon, ship-builder; for an improvement in the form, formation and construction of ships and other vessels of war, and ships and other vessels of commerce, and of sloops, barges, and other vessels any otherwise employed. Dated September 6.

To Joseph Manton, of Davies-street, Berkeley-square, London, gun-maker; for his improvement in double-barrelled guns. Dated September 15.

To Isaiah Birt, of Plymouth Dock, in the county of Devon, gent.; for his black paint, composed chiefly of earthy and mineral substances, which will be beneficial to our navy and the shipping interest at large, being particularly calculated to preserve wood, and prevent rust in iron, and may be applied to all purposes for which paint in general is used. Dated September 18.

To Marc Isambard Brunel, of Portsea, in the county of Southampton, gent.; for his new mode of cutting veneers or thin boards. Dated September 23.

METEORO-

METEOROLOGICAL TABLE,
BY MR. CAREY, OF THE STRAND,
For September 1806.

Days of the Month.	Thermometer. 8 o'Clock, Morning.	Noon.	11 o'Clock, Night.	Height of the Barom. Inches.	Degrees of Dryness by Leslie's Hygrometer.	Weather.
Aug. 27	60°	68°	56°	29·56	47°	Fair
28	57	68	59	·60	40	Fair
29	56	68	55	·30	0	Rain
30	55	66	55	·70	26	Showery
31	56	69	56	·80	37	Fair
Sept. 1	57	72	59	·92	46	Fair
2	59	68	60	30·11	40	Cloudy
3	60	69	60	·01	40	Fair
4	60	68	58	29·75	26	Showery
5	57	66	59	·98	42	Fair
6	58	69	61	30·00	46	Fair
7	63	71	64	29·98	41	Fair
8	61	71	54	30·00	44	Fair
9	55	54	50	29·55	0	Rain, with thunder
10	53	64	50	·76	31	Fair
11	50	59	49	·70	35	Fair
12	51	59	48	30·00	15	Fair
13	51	61	51	·12	33	Fair
14	54	63	54	·01	16	Fair
15	56	56	49	29·95	30	Rain
16	48	60	54	30·16	51	Fair
17	53	64	53	·25	49	Fair
18	55	65	57	·25	22	Fair
19	56	67	59	·26	30	Fair
20	59	68	61	·21	36	Fair
21	60	67	55	·19	25	Cloudy
22	55	69	55	29·99	41	Fair
23	56	65	54	·99	25	Cloudy
24	54	57	47	30·10	20	Cloudy
25	47	60	51	·22	25	Fair
26	44	64	52	·24	56	Fair

N. B. The barometer's height is taken at noon.

INDEX TO VOL. XXV.

Spinning

END OF THE TWENTY-FIFTH VOLUME.

ERRATUM.—In the Title of Article I. of this Volume insert, *By M. Deyeux.*

Printed by R. Taylor and Co., 38, Shoe Lane, Fleet Street.

Fig. 26.

Fig. 27.

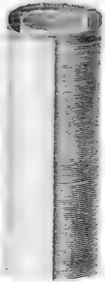


Fig. 28.

Fig. 29.

Fig. 30.

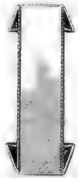


Fig. 31.

Fig. 32.

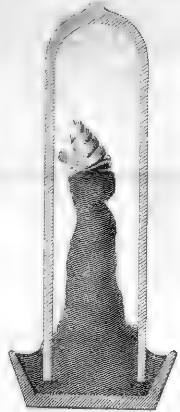


Fig. 33.

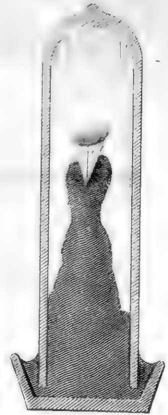
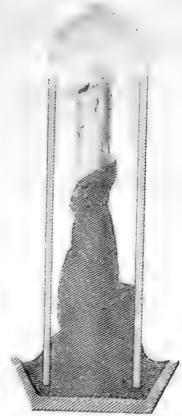
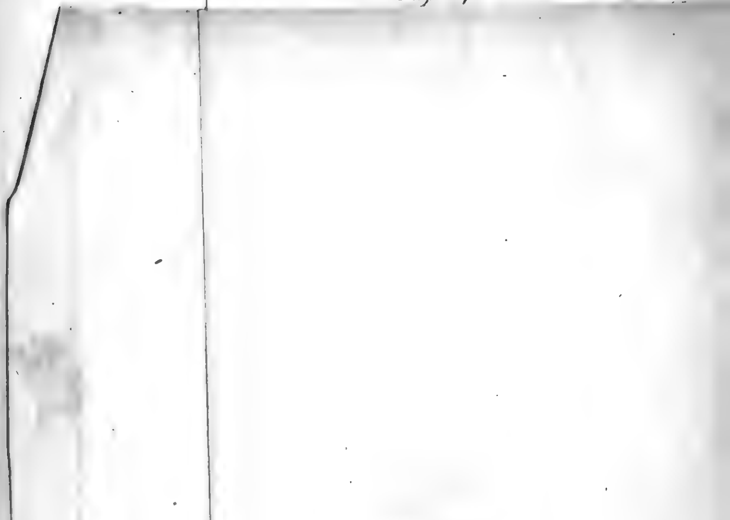


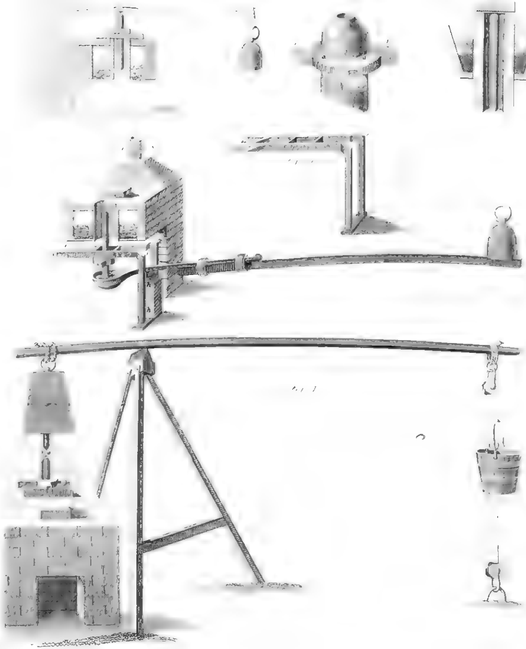
Fig. 34.

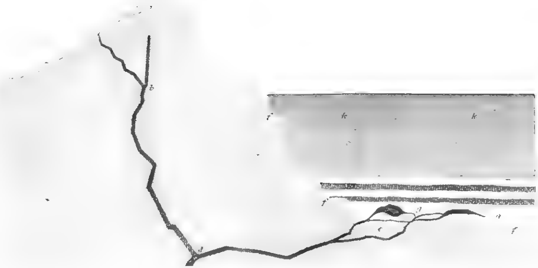


Phil. Mag. Pl. II. Vol. XXV.

Fig. 37.







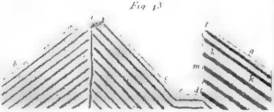
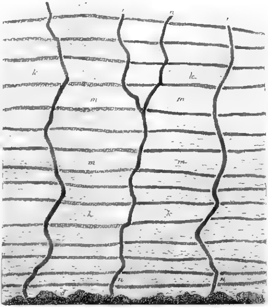
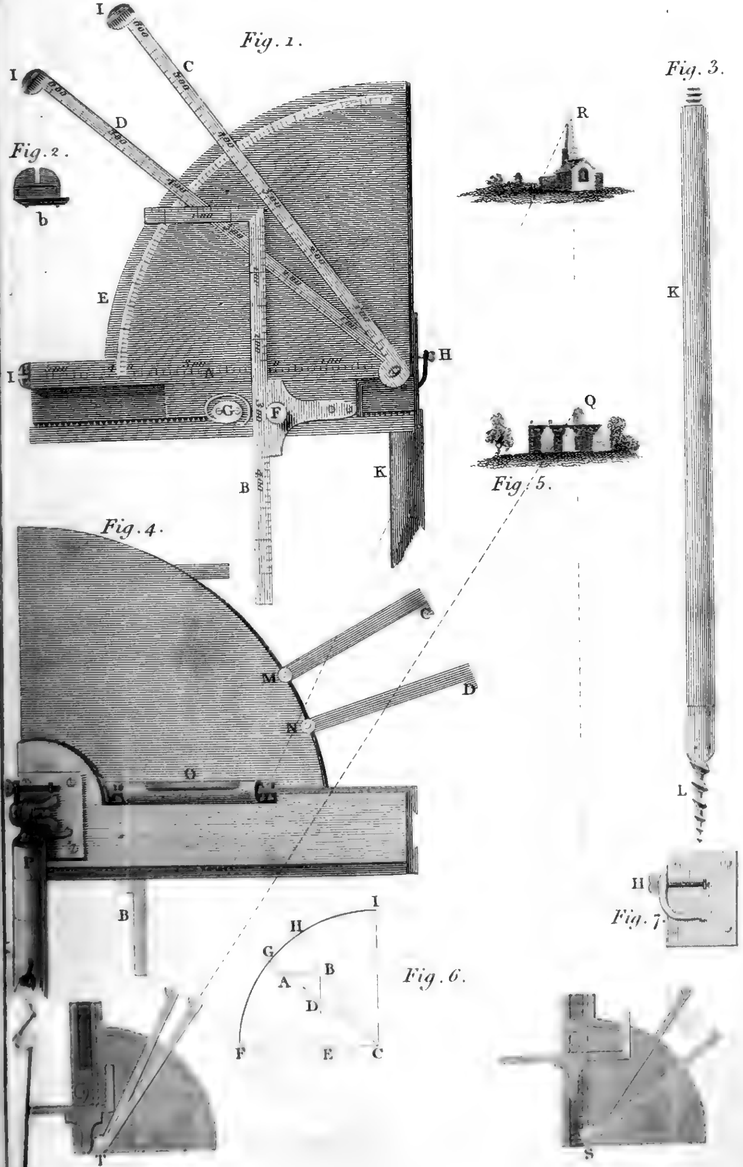


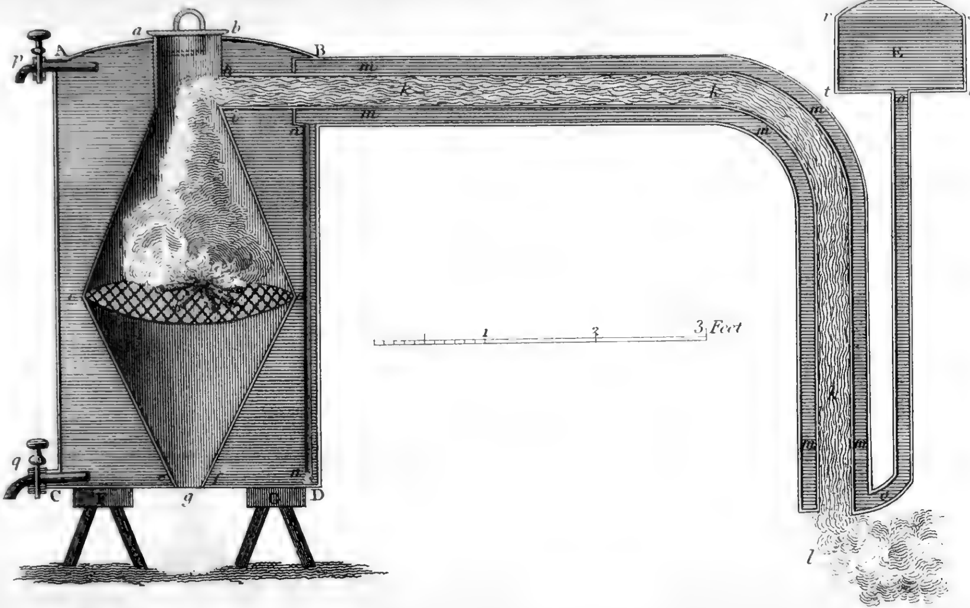
Fig. 13



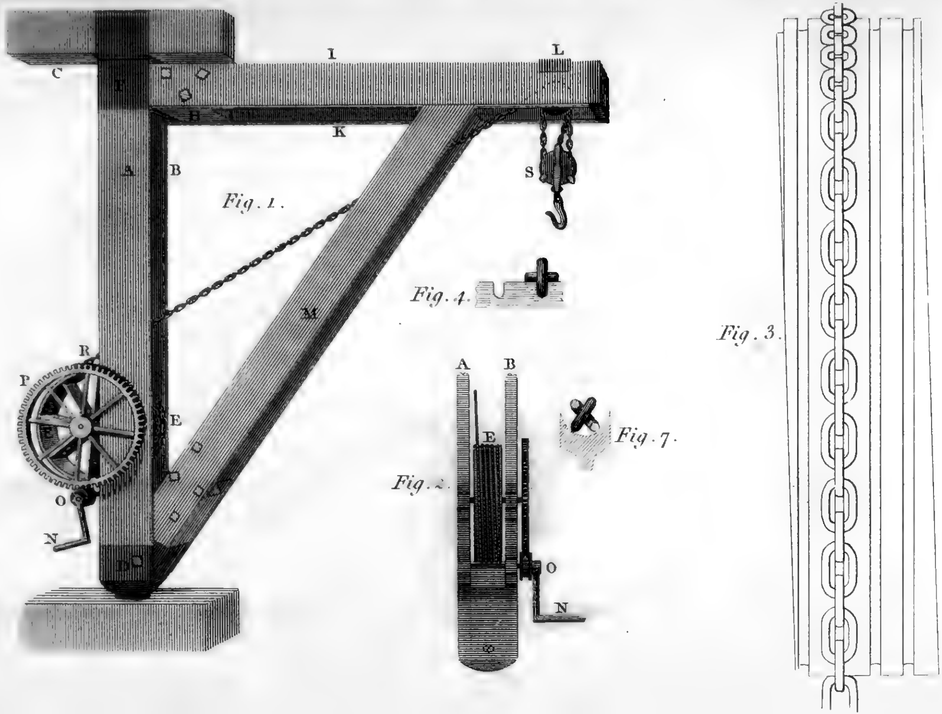
Mr. R. Salmon's Geometrical Quadrant & Staff.



Montgolfier's Calorimeter.



Mr. Gilbert Gilpin's Crane for raising Weights.



S. Porter sculp.

Mr. Peter Herbert's Book-Case Bolt.

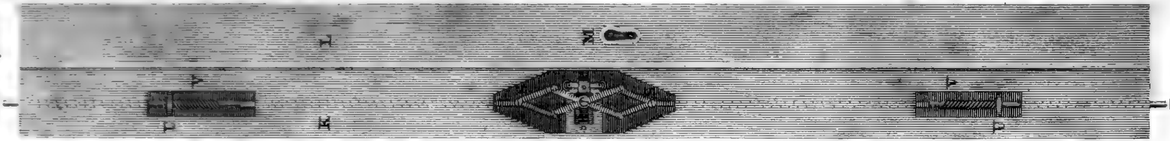


Fig. 3.

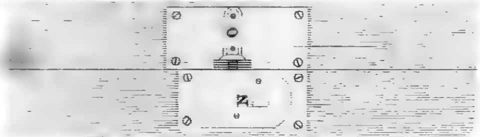


Fig. 4.

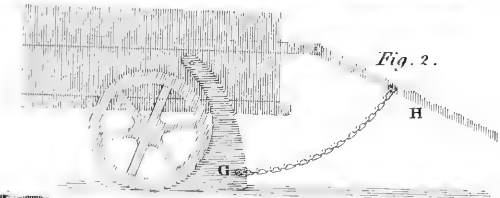


Fig. 2.

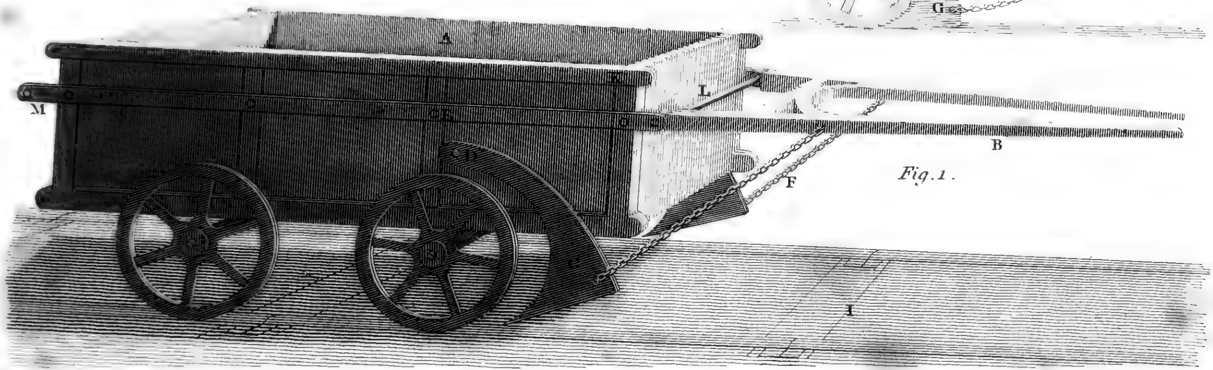


Fig. 1.

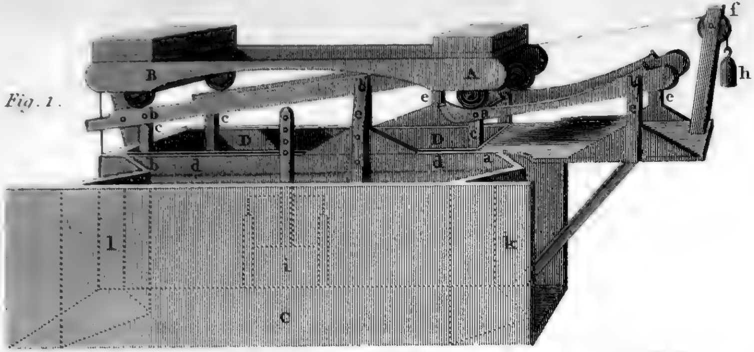
Mr. Le Caan's Check to Carriage Wheels.

99

S. Porter sculp.

Mr. Salmon's Improvement on Canal Locks.

Fig. 1.



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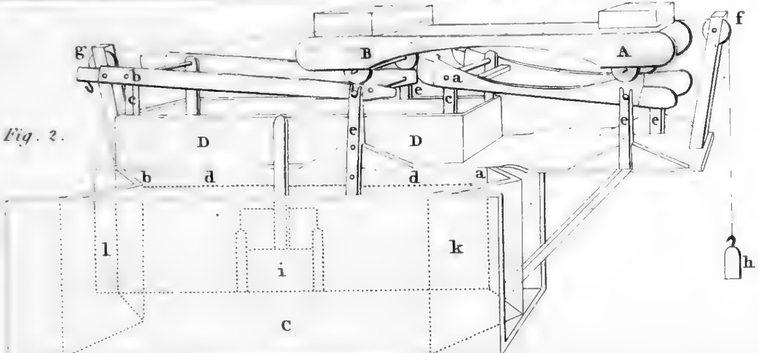
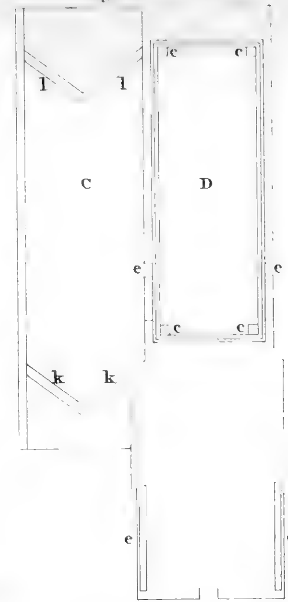


Fig. 3.



S. Porter sculp

GALVANISM.

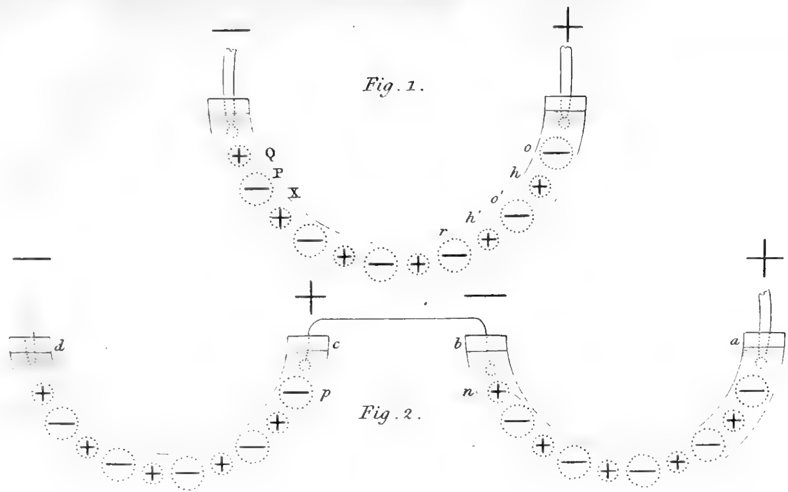


Fig. 1.

Fig. 2.

Head of Silex encrusted with Calcedony.

Fig. 1.

Fig. 2.